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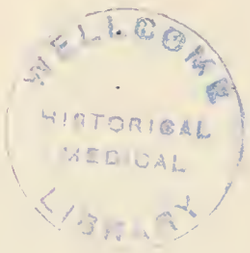

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DUKE, ALLEN, M.D., M.R.C.S. Eng., L.S.A., Dover.

DUNCAN, THOMAS, M.D., L.R.C.P. Lond., M.R.C.S. Eng., L.M., L.S.A., 16, The Green, Richmond, Surrey.

DUNCAN, GEORGE, M.D., L.R.C.S. Edin., Lochalsh, Ross-shire.

DYER, SAMUEL S., M.D., L.R.C.P. Lond., M.R.C.S., Eng., L.S.A., Ringwood, Hants.

EDE, JOHN R., M.D., F.R.C.S. Eng., L.S.A., Surg. Islington Parochial Infirm., 167, Hemingford-road, Barnsbury, N.

EDGCOME, JAMES, M.D., M.R.C.S. Eng., L.S.A., 24, Brunswick-square, W.C.

EDMUNDS, JAMES, M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.S.A., 4, Fitzroy-square, W.

EDWARDS, JOHN, M.D., M.R.C.S. Eng., Sparkbrook, Birmingham.

ELLIOTT, GEORGE S., M.D., L.S.A., Southwell, Notts.

ELLIS, EDWARD, M.D., M.R.C.S. Eng., M. & L.S.A., Phys. Victoria Hosp. for Child. and Samarit. Hosp. for Wom. and Child., 118, Warwick-street, Belgravia, S.W.

ERSKINE, WILLIAM, M.D., L.F.P.S. Glasg., Kincardine, Perthshire.

EVANS, NICHOLL, M.D., M.R.C.S. Eng., Cheshunt, Herts.

EVERS, CHARLES, M.D., M.R.C.S. Eng., L.S.A., St. Austin's Mount, Grassendale, Liverpl.

EVES, AUGUSTUS, M.D., F.R.C.S. Eng., Cons. Surg., Cheltenham Gen. Hosp., Cheltenham.

FAIRLESS, W. DEAN, M.D., M.R.C.S. Eng., Longdales, Bothwell, N.B.

FARQUHARSON, DUNCAN, M.D., L.R.C.P. Edin., L.R.C.S. Edin., Tillicoultry, Stirling.

FAWCUS, HENRY R., M.D., M.R.C.S. Eng., L.S.A., Flodden-lodge, Ford, Coldstream.

FEGAN, RICHARD, M.D., L.K.Q.C.P. Irel., L.R.C.S.I., L.M. Dub., Charlton, Kent.

FENTON, JOHN, M.D., L.R.C.P. Edin., F.R.C.S. Eng., L.S.A., 16, Mornington-terrace, Liverpool.

FINCH, ROBERT, M.D., M.R.C.S. Eng., L.S.A., Med. Off. of Health for Charlton, Blackheath, S.E.

FISHER, LUKE, M.D., M.R.C.S. Eng., L.S.A., Lytham, Preston, Lancashire.

FLEMING, HANS, M.D., L.R.C.S.I., L.M., Omagh, Co. Tyrone.

FLEMMING, THOMAS H., M.D., L.F.P.S. Glasg., Freshford, Bath.

FLETCHER, JOHN S., M.D., M.R.C.S. Eng., L.S.A., 8, Lever-street, Manchester.

FOOTE, C. N., M.D., M.R.C.S. Eng., L.S.A., 8, Chester-road, Bishopwearmouth, Sunderland.

FORD, JAMES, M.D., M.R.C.S. Eng., L.S.A., Chulmleigh, North Devon.

FOX, CHARLES H., M.D., M.R.C.S. Eng., Med. Superintendent, Brislington House, Bristol.

FOX, LUTHER O., M.D., L.R.C.P. Edin., F.R.C.S. Eng., L.S.A., Broughton, Stockbridge, Hants.

FRAIN, JOSEPH, M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.S.A., Hon. Surg. South Shields and Westoe Disp., Frederick-street, South Shields.

GARDEN, J., M.D., 4, Clifton-road, St. John's Wood, N.W.

GARDENER, WILLIAM S., M.D., M.R.C.S. Eng., L.A.H. Dub., L.M., Workhouse, Cork.

GARDINER, GIDEON G., M.D., M.R.C.S. Eng., Brooke-house Private Lunatic Asylum, Upper Clapton, N.E.

GARLICK, JOHN W., M.D., L.R.C.P. Lond., M.R.C.S. Eng., L.S.A., 6, Lord-street, Halifax.

GARSTANG, WALTER, M.D., L.R.C.P. Lond., L.R.C.P. Edin., F.R.C.S. Edin., L.M., 2, France-street, Blackburn.

GEDDES, JOHN, M.D., L.R.C.S. Edin., 37, Union-place, Aberdeen.

GILES, GEORGE F., M.D., M.R.C.S. Eng., L.S.A., Springfield-road, St. Leonard's-on-Sea.

GILL, JOHN B., M.D., M.R.C.S. Eng., L.S.A., 4, Camden-crescent, Dover.

GODDARD, R. WALTER, M.D., M.R.C.S. Eng., L.M., L.S.A., 53, Connaught-terrace, W.

GODFREY, BENJAMIN, M.D., M.R.C.S. Eng., L.S.A., F.R.A.S., Carlton House, Enfield, Middlesex.

GODWIN, ASHTON, M.D., L.R.C.P. Lond., M R.C.S. Eng., L.M., L.S.A., 11, Pelham-crescent, Brompton, S.W.

GOLDER, JAMES, M.D., L.R.C.S. Edin., 101, Crown-street, Glasgow.

GOLDSMITH, JOHN, M.D., M.R.C.S. Eng., L.S.A., Worthing, Sussex.

GOODDAY, HORATIO, M.D., M.R.C.S. Eng., L.S.A., M.S.A., 66, Kensington-park-terr., W.

GOODMAN, CHARLES R., M.D., M.R.C.S. Eng., L.S.A., 205, York-st., Cheetham, Manchester.

GORDON, C. A., M.D., L.R.C.S. Edin., C.B., Dep. Insp.-Gen. Army Hosp., Portsmouth.

GOSS, S. DAY, M.D., L.R.C.P. Edin., F.R.G.S., 111, Kennington-park-road, S.

GOYDER, DAVID, M.D., L.R.C.S., Edin., 31, Horton-road, Bradford, Yorkshire.

GRAMSHAW, JAMES H., M.D., M.R.C.S., Eng., L.S.A., Surg. Infirm. and Disp., 25, King-street, Gravesend.

GRAVES, FREDERICK G., M.D., M.R.C.S. Eng., L.S.A., 1, Westbourne-terrace-villas, Westbourne-square, W.

GRAY, THOMAS S., M.D., M.R.C.S. Eng., L.S.A., Lansdowne House, 340, Essex-road, N.

GREENHALGH, ROBERT, M.D., M.R.C.P. Lond., M.R.C.S. Eng., Phys. Acc. and Lect. on Dis. of Wom. and Children, St. Barthol. Hosp., Cons. Phys. Acc. City of Lond. Lying-in Hosp., Cons. Phys. Samarit. Hosp. for Wom. and Child., 77, Grosvenor-street, W.

GREENWELL, BAKER, M.D., M.R.C.S. Eng., L.S.A., 49, New Finchley-road, N.W.

GREENWOOD, MAJOR, M.D., M.R.C.S. Eng., L.M., L.S.A., 26, Queen's-road, Dalston, N.E.

GRIFFITH, JOHN T., M.D., F.R.C.S. Eng., L.S.A., Talfourd House, Camberwell, S.

GRIFFITH, S., M.D., M.R.C.S. Eng., L.S.A., Portmadoc, Carnarvon.

GRIFFITH, SAMUEL C., M.D., M.R.C.S. Eng., M. and L.S.A., 65a, Harley-street, W.

GRIFFITHS, FRANCIS T., M.D., M.R.C.S. Eng., L.M., L.S.A., B.L. Univ. France, F.S.A., Lect. on Physiol. and Pathol. Anat. Sheffield Med. Sch., 12, Tudor-street, Surrey-street, Sheffield.

GROVE, WILLIAM R., M.D., M.R.C.S. Eng., L.S.A., Med. Off. of Health, St. Ives, Hunts.

GWYNN, S. TAYLEUR, M.D., M.R.C.S. Eng., L.S.A., Whitchurch, Salop.

HADDEN, HENRY, R., M.D., F.R.C.S.I., L.M., Clonakilty, Co. Cork.

HALL, EGERTON F., M.D., L.R.C.P. Edin., M.R.C.S. Eng., The Ash Trees, Prescot, Lancash.

HARDING, CHARLES F., M.D., M.R.C.S. Eng., Whittlesea, Cambridgeshire.

HARLAND, HENRY, M.D., M.R.C.S. Eng., L.S.A., L.M. Glasg., Mayfield, Sussex.

HARPER, HENRY L., M.D., M.R.C.S. Eng., L.S.A., Sussex House, Hammersmith.

HARRIS, CHARLES, M.D., M.R.C.S. Eng., L.S.A., Northiam, Sussex.

HARRISON, J. BOWER, M.D., M.R.C.P. Lond., F.R.C.S. Eng., L.S.A., Higher-Broughton, Manchester.

HARRISON, WILLIAM, M.D., L.R.C.P. Edin., M.R.C.S. Lond., L.M., L.S.A., Gargrave, Leeds.

HARRISON, CHARLES, M.D., M.R.C.S. Eng., L.S.A., Surg. Lincoln Gen. Disp., Med. Off. of Health, 26, Melville-street, Lincoln.

HARRISON, A. R., M.D., M.R.C.P. Edin., M.R.C.S. Eng., L.S.A., Pembroke House, Hackney, N.E.

HAYDON, N. J., M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.S.A., Bovey-Tracey, Devon.

HAYWARD, JOHN W., M.D., M.R.C.S. Eng., L.S.A., 117, Grove-street, Liverpool.

HAYWARD, JOHN W., M.D., M.R.C.S. Eng., L.S.A., 7, South-parade, Leeds.

HEAD, THOMAS, M.D., F.R.C.P. Edin., M.R.C.S. Eng., L.S.A., Leathes-house, Penrith, Cumberland.

HEARDER, GEORGE J., M.D., L.R.C.S. Edin., Med. Supt. Joint Counties Asyl., Carmarthen.

HECKSHER, MARTIN, M.D., M.D. Berlin, M.R.C.P. Lond., 245, Oxford-road, Manchester.

HELSHAM, H., M.D., F.R.C.S. Eng., L.M., L.S.A., 4, Park-place, Brixton-road, S.W.

HENDERSON, WILLIAM, M.D., L.M. Edin., 17, Rose-terrace, Perth.

HENDERSON, JOHN, M.D., F.R.C.S. Edin., Surg. Leith Hosp., 7, John's-place, Leith.

HENRY, ALEXANDER, M.D., M.R.C.S. Eng., L.S.A., Surg. Inst. for the Blind, Deaf, and Dumb, 15, Alfred-street, Bath.

HENTY, GEORGE, M.D., M.R.C.S. Eng., 34, Hilldrop-road, Tufnell-park, N.

HETLEY, FREDERIC, M.D., F.R.C.S. Eng., L.S.A., L.M., Westow-hill, Upper Norwood, Surrey.

HIGGINS, CHARLES H., M.D., M.R.C.P. Lond., F.R.C.S. Eng., L.S.A., Alfred House, Birkenhead.

HIGHMORE, NATHANIEL J., M.D., M.R.C.S. Eng., L.S.A., Bradford-on-Avon.

HIGHMORE, WILLIAM, M.D., M.R.C.S. Eng., L.S.A., Sen. Phys. and Surg. Yeatman Hosp., Sherborne, Dorsetshire.

HILL, JOHN D., M.D., M.R.C.S. Eng., L.S.A., Surg. Roy. Free Hosp., 17, Guilford-st., W.C.

HILL, SAMUEL, M.D., L.R.C.P. Edin., L.R.C.S.I., L.S.A., L.M., 22, Mecklenburgh-sq., W.C.

HODGSON, WILLIAM J., M.D., M.R.C.S. Eng., L.S.A., Sutton-St.-Mary, Wisbeach, Linesh.

HODGSON, ROBERT W., M.D., L.R.C.P. Edin., M.R.C.S., Eng., L.S.A., Northallerton, Yorkshire.

HOLCOMBE, CHARLES A., M.D., M.R.C.S. Eng., L.S.A., 29, South-hill-road, Liverpool.

HOLMAN, HENRY M., M.D., F.R.C.S. Eng., L.S.A., Hurstpierpoint, Sussex.

HOLMAN, C., M.D., M.R.C.S. Eng., L.S.A., Reigate, Surrey.

HOOD, PETER, M.D., L.S.A., 15, Lower Seymour-street, W.

HOOD, WHARTON P., M.D., M.R.C.S. Eng., L.S.A., 65, Upper Berkeley-st., Portman-sq., W.

HORNER, THOMAS, M.D., L.R.C.P. Edin., M.R.C.S. Eng., Newgate, Chester.

HOWARD, JAMES F., M.D., M.R.C.S. Eng., L.S.A., Shaw, Lancashire.

HUBERT, T. K., M.D., M.R.C.S. Eng., L.S.A., Billingshurst, Sussex.

HUGHES, ROBERT, M.D., M.R.C.S. Eng., L.S.A., L.M., Conway, Carnarvon.

HUGHES, JAMES S., M.D., L.R.C.S.I., L.M., L.A.H. Dub., 77, Roscommon-street, Liverpool.

HULME, JAMES D., M.D., M.R.C.S. Eng., L.S.A., Great Wigston, Leicester.

HUMBY, EDWIN, M.D., M.R.C.S. Eng., L.S.A., 83, Hamilton-terr., St. John's-wood, N.W.

HUTCHISON, WILLIAM, M.D., L.F.P.S. Glasg., L.M., Loughborough, Leicestershire.

ILES, F. H. WILSON, M.D., M.R.C.S. Eng., L.M., L.S.A., Watford, Herts.

INGHAM, AMOS, M.D., M.R.C.S. Eng., L.S.A., Keighley, Yorkshire.

INGLE, ROBERT N., M.D., L.R.C.P. Lond., F.R.C.S. Eng., L.S.A., Surg. St. Mary's Hosp., Pendleton, Manchester.

IRWIN, WILLIAM C., M.D., M.R.C.P. Lond., L.R.C.P. Edin., M.R.C.S. Eng., L.S.A., Belvoir-street, Leicester.

IRWIN, JARRITT, M.D., L.F.P.S. Glasg., L.M., L.A.H. Dub., Tullow, Co. Carlow.

JACKSON, J. HUGHLINGS, M.D., M.R.C.P. Lond., M.R.C.S. Eng., Phys. Lond. Hosp., Lect. on Physiol. Lond. Hosp. Med. Coll., Phys. Hosp. for Epilepsy and Paralysis, 28, Bedford-place, Russell-square, W.C.

JAMES, M. PROSSER, M.D., M.R.C.P. Lond., M.R.C.S. Eng., L.M., L.S.A., Sen. Phys. City Disp., 18, Dover-street, Piccadilly, W.

JAMES, ALFRED, M.D., M.R.C.S. Eng., L.S.A., Perry Vale, Forest Hill, Kent.

JEFFERY, GEORGE AUGUSTUS, M.D., M.R.C.S. Eng., L.S.A., Phys. Conv. Hosp., Trinity House, Eastbourne.

JENCKEN, FERDINAND E., M.D., M.R.C.P. Lond, L.M., 27, Adelaide-street, Kingstown, Co. Dublin.

JENNINGS, ROBERT, M.D., L.S.A., L.R.C.S. Edin., L.M., Eastry, Kent.

JEPHCOTT, SAMUEL T., M.D., M.R.C.S. Eng., L.S.A., 86, Foregate-street, Chester.

JEPSON, O., M.D., M.R.C.S. Eng., L.S.A., Med. Superint. City of London Lunat. Asyl., Stone, Dartford, Kent.

JOHNSON, HORACE E., M.D., L.R.C.P. Edin., L.S.A., 8, Ovington-square, Brompton, S.W.

JONES, WALTER, M.D., M.R.C.S. Eng., L.M., L.S.A., Surg. Western City Disp., 45, Finsbury-square, E.C.

JONES, EDWARD, M.D., B.L. Univ. France, M.R.C.S. Eng., L.S.A., Surg. Sydenham Disp., The Park, Sydenham, Kent.

JONES, THOMAS, M.D., M.R.C.S. Eng., L.S.A., Phys. Ross Disp., 8, Church-street, Ross, Herefordshire.

JONES, W. GOODALL, M.D., M.R.C.S. Eng., L.M., L.S.A., 6, Prince Edwin-street, Liverpool.

JONES, PODMORE W. H., M.D., M.R.C.S. Eng., L.S.A., F.L.S., 58, Rodney-street, Liverpool.

KEALY, JOHN R., M.D., M.R.C.S. Eng., L.S.A., Surg. Portsmouth, Portsea, and Gosport Hosp., Ashley House, Gosport.

KEILLER, ALEXANDER, M.D., F.R.C.P. Edin., L.R.C.S. Edin., F.R.S. Edin., Phys. Roy. Matern. Hosp. and Hosp. for Sick Child., Phys. Acc. Roy. Pub. Disp. Edin., Lect. on Midw. and Dis. of Women and Child. Surg. Hall Edin., Exam. in Midw. Univ. St. And., 21, Queen-street, Edinburgh.

KERNOT, C. M., M.D., M.R.C.S. Eng., L.M., Chrisp-street, Poplar, E.

KERSEY, ROBERT C., M.D., M.R.C.S. Eng., L.S.A., Littlebourne, Wringham, Kent.

KINAHAN, —, M.D., Carrick-on-Suir, Co. Tipperary.

KING, THOMAS K., M.D., M.R.C.S. Eng., L.S.A., L.A.H. Dub., 2, Portland-place, South Camberwell, S.

KING, THOMAS W., M.D., M.R.C.S. Eng., 159, Camberwell-road, S.

KINGSFORD, CHARLES D., M.D., M.R.C.S. Eng., L.S.A., Surg. Lond. Orph. Asyl. Clapton, Surg. Roy. Brit. Asy. for Deaf and Dumb Females, Clapton, Upper Clapton, N.E.

LAKE, WILLIAM C., M.D., M.R.C.S. Eng., L.S.A., Surg. Teignmouth, Dawlish, and Newton Infirm., Teignmouth, Devon.

LAWLOR, JEREMIAH, M.D., L.R.C.P. Edin., L.F.P.S. Glasg., L.M., L.A.H. Dub., Phys. Queenstown Fev. and Gen. Hosp., Queenstown, Co. Cork.

LAWRENCE, SAMUEL, M.D., L.R.C.S. Edin. Montrose.

LEE, HERBERT G., M.D., A.A. Oxon, M.R.C.S. Eng., L.S.A., Thame, Oxon.

LEE, MATTHEW, M.D., M.R.C.S. Eng., St. Andrew's-villas, Lister-hills, Bradford, Yorkshire.

LEONARD, PETER, M.D., M.R.C.P. Lond., L.R.C.S. Edin., Insp.-Gen. Hosps. and Fleets, Plaisaunce, Upper Norwood, S.

LINDSAY, J. MURRAY, M.D., L.R.C.S. Edin., L.S.A., Med. Superint. Female Depart. Middlesex Co. Asyl., Hanwell.

LIPSCOMB, J. T. N., M.D., F.R.C.S. Eng., L.S.A., Surg. Gen. Disp., St. Albans, Herts.

LISTER, BRYAN, M.D., M.R.C.S. Eng., L.S.A., Littleborough, Manchester.

LLOYD, EDWIN, M.D., M.R.C.S. Eng., L.S.A., 59, Bath-terrace, Worksop, Notts.

LLOYD, THOMAS, M.D., F.R.C.S. Eng., L.S.A., Norfolk-villa, West-hill, St. Leonards-on-Sea

LOCKING, JOHN, M.D., L.R.C.S. Edin., 17, Connaught-square, W.

LONG, RICHARD, M.D., A.M. St. And., L.A.H. Dub., L.M., Phys. Camphill Fev. Hosp., Arthurstown, New Ross, Co. Wexford.

LUSH, J. A., M.D., M.R.C.P. Lond., M.R.C.S. Eng., L.S.A., M.P. for Salisbury, Fisherton House, Salisbury.

LYELL, DAVID, M.D., L.R.C.S. Edin., 23, Tay-street, Dundee.

LYNES, EDWARD, M.D., M.R.C.S. Eng., L.M., L.S.A., Surg. Coventry and Warwicksh. Hosp., 9, Priory-row, Coventry.

LYSTER, CHAWORTH E., M.D., L.R.C.S.I., L.M., L.S.A., Surg. Toxteth-pk. Fev. Hosp., 20, Belvidere-road, Prince's-park, Liverpool.

MACKENZIE, J., M.D., L.R.C.S. Edin., Staff Assist.-Surg. Army, Shorncliffe.

MACKINDER, DRAPER, M.D., F.R.C.S. Edin., M.R.C.S. Eng., L.S.A., Gainsborough.
MACKINTOSH, CHARLES H., M.D., L.R.C.P. Lond., M.R.C.S. Eng., L.S.A., Morden Hall, Warren-road, Torquay.
MACKINTOSH, ALEXANDER, M.D., L.F.P.S. Glasg., Phys. Supt. Roy. Lunat. Asylum, Gartnavel, Glasgow.
MACLEOD, WILLIAM, M.D., F.R.C.P. Edin., L.R.C.S. Edin., Ben Rhydding, Otley, Yorksh.
MADDEN, LEWIS P., M.D., M.R.C.P. Lond., M.R.C.S. Eng., J.P., Jacobstowe, Devonshire.
MADGE, H. M., M.D., M.R.C.S. Eng., L.S.A., 32, Fitzroy-square, W.
MAGILL, MARTIN, M.D., F.R.C.S. Eng., R.N. 39, Chepstow-villas, Bayswater, W.
MAJOR, HARRY P., M.D., M.R.C.S. Eng., M. and L.S.A., High-street, Hungerford, Berks.
MANFORD, ROBERT A., M.D., F.R.C.S. Edin., L.F.P.S. Glasg., J.P. for Co. Inverness, 64, Academy-street, Inverness.
MARSDEN, ALEXANDER, M.D., M.R.C.S. Eng., Surg. Roy. Free Hosp., Surg. Cancer Hosp., 65, Lincoln's-inn-fields, W.C.
MARSTON, CHARLES H., M.D., L.R.C.P. Edin., M.R.C.S. Eng., Phys. North Wilts Disp., 27, Long-street, Devizes, Wilts.
MARSTON, JEFFERY A., M.D., M.R.C S. Eng., L.S.A., Asst.-Surg. Roy. Artillery.
MARTIN, FREDERICK, M.D., M.R.C.S. Eng., L.S.A., Phillimore House, Beckenham, Kent, S.E.
MARTIN, THOMAS, M.D., M.R.C.S. Eng., L.S.A., Pendlebury, Manchester.
MASSEY, ISAAC, M.D., L.R.C.P. Edin., F.R.C.S. Eng., L.S.A., Surg. Nottingham Co. Gaol, Cons. Surg. Nottingham Disp., Wellington-circus, Nottingham.
MATTHEWS, JOHN, M.D., L.S.A., Phys. Lond. Female Penitent. Pentonville, 4, Mylne-street, E.C.
MAUGHAM, WILLIAM, M.D., M.R.C.S. Eng., L.S.A., L.M. Glasg., Northgate House, Carnarvon.
MAUND, HENRY, M.D., M.R.C.S. Eng., L.S.A., Sandown, Isle of Wight.
MAY, HERMAN S., M.D., M.R.C.S. Eng., Summerlands, Exeter.
McCONVILLE, JOHN, M.D., L.R.C.S. Edin., 476, Argyle-street, Glasgow.
McEWEN, WILLIAM, M.D., M.R.C.P. Lond., L.R.C.S. Edin., Surg. Chester Castle, 27, Nicholas-street, Chester.
McGOWN, JOHN, M.D., L.F.P.S. Glasg., Millport, Isle of Cumbræ, N.B.
McINTYRE, JOHN, M.D., L.R.C.S. Edin., L.S.A., Odiham, Hants.
McLEAN, HUGH, M.D., M.R.C.S. Eng., L.S.A., Corbridge, Northumberland.
MELLER, CHARLES M., M.D., M.R.C.S. Eng., L.S.A., Matlock Bath, Derbyshire.
MENZIES, WILLIAM, M.D., F.R.C.S. Edin., 3, Lothian-road, Edinburgh.
MERRYWEATHER, HENRY, M.D., M.R.C.S. Eng., Lect. on Dent. Surg. Sheffield Sch. of Med., 7, Surrey-street, Sheffield.
MILES, EDWIN J., M.D., M.R.C.S. Eng., L.S.A., Gillingham, Dorsetshire.
MILLER, CLAUDIUS M., M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.S.A., Claremont Villa, Stoke Newington-road, N.E.
MILLER, GEORGE, M.D., M.R.C.S. Eng., L.S.A., Sidmouth, Devon.
MILLS, W. PARTRIDGE, M.D., L.R.C.P. Lond., M.R.C.S. Eng., L.S.A., Ipswich.
MINTER, JOHN M., M.D., F.R.C.S. Eng., L.S.A., Dep. Insp.-Gen. Hosps. and Fleets, Denbigh Lodge, Southsea, Hants.
MOFFAT, THOMAS, M.D., L.R.C.S. Edin., F.R.A.S., F.G.S., Hawarden, Flintshire.
MOON, HENRY, M.D., M.R.C.P. Lond., M.R.C.S. Eng., L.S.A., Phys. Sussex Co. Hosp., 9, Old Steyne, Brighton.
MOORE, GEORGE, M.D., M.R.C.P. Lond., M.R.C.S. Eng., L.S.A., Hastings.
MOORE, DANIEL, M.D., M.R.C.S. Eng., L.S.A., 3, Wellington-square, Hastings.
MORGAN, WILLIAM W., M.D., M.R.C.S. Eng., L.S.A., Newport, Monmouthshire.
MORGAN, SAMUEL, M.D., M.R.C.S. Eng., L.S.A., 46, Rivers-street, Bath.
MORRIS, EDWIN, M.D., F.R.C.S. Eng., L.S.A., High-street, Spalding, Lincolnshire.
MOTT, CHARLES, M.D., M.R.C.S. Eng., L.S.A., Walton-on-Thames.
MURRAY, JOHN C., M.D., L.R.C.S. Edin., L.M., L.R.C.P. Edin., 20, Newgate-street, Newcastle-on-Tyne.
MURRAY, HENRY ASH, M.D., L.S.A., Oaken, Wolverhampton.
NASH, THOMAS L., M.D., M.R.C.S. Eng., Surg. 63rd Regt.
NEAL, JAMES, M.D., M.R.C.S. Eng., L.S.A., Surg. Birmingham Lying-in Hosp., 85, Newhall-street, Birmingham.
NEEDHAM, FREDERICK, M.D., M.R.C.P. Edin., M.R.C.S. Eng., L.M., L.S.A., Res. Med. Superint. York Lunat. Asyl., Bootham, York.
NESFIELD, STEPHEN, M.D., M.R.C.S. Eng., L.S.A., Surg. St. Mary's Hosp., 342, Stretford-road, Manchester.
NICHOLAS, GEORGE E., M.D., M.R.C.S. Eng., L.M., L.S.A., Med. Off. of Health for Wandsworth, 4, Church-row, Wandsworth, S.W.
NICHOLLS, JAMES, M.D., L.R.C.P. Lond., L.R.C.P. Edin., M.R.C.S. Eng., L.M., L.S.A., Chelmsford.
NICHOLLS, JOHN F., M.D., M.R.C.S. Eng., Surg. Roy. Wilts Militia, Devizes.

NIVEN, J., M.D., 28, Chepstow-place, Bayswater, W.

NORRIS, WILLIAM, M.D., L.S.A., Stourbridge, Worcestershire.

O'CONNOR, WILLIAM, M.D., M.R.C.S. Eng., L.S.A., Sen. Phys. Royal Free Hosp., 30, Upper Montagu-street, Montagu-square, W.

OGDEN, JAMES, M.D., M.R.C.P. Lond., M.D. Erlang., M.A., Ph.D., Phys. St. Mary's Hosp., Ardwick Villa, Manchester.

O'NEILL, EDWARD J., M.D., L.R.C.S.I., L.A.H. Dub., 27, North Earl-street, Dublin.

OSBORNE, JOHN, M.D., F.R.C.S. Eng., L.S.A., Bitterne, Southampton.

OSBORNE, HENRY, M.D., M.R.C.S. Eng., L.S.A., Phys. Islington Disp., 3, Gibson-square, Islington, N.

OWEN, HARVEY K., M.D., F.R.C.S. Eng., M. and L.S.A., Surg. South London Disp., 4, Binfield-place, Clapham-road, S.

PAGE, ALEXANDER H., M.D., L.K.Q.C.P. Irel., M.R.C.S. Eng., Jun. Surg. Royal Albert Hosp., 138, Duckworth-street, Over-Darwen, Lancashire.

PAINTER, R. BUDD, M.D., F.R.C.S. Eng., M. and L.S.A., 4, Beaufort-gardens, Brompton-road, S.W.

PALMER, CHARLES, M.D., M.R.C.S. Eng., L.S.A., Ormskirk, Manchester.

PARKER, THEOPHILUS R. B., M.D., M.R.C.S. Eng., L.S.A., The Vicarage, Abbotsbury, Dorset.

PARKER, WILLIAM, M.D., L.F.P.S. Glasg., L.S.A., Med. Off. of Health for Bermondsey, 133, Grange-road, Bermondsey, S.E.

PARRATT, JAMES, M.D., M.R.C.S. Eng., M. and L.S.A., Mem. Court of Exam. Apoth. Soc., 18, Mount-street, W.

PART, JAMES, M.D., F.R.C.S. Eng., L.S.A., Surg.-Oculist to Artists' Ann. Fund, 89, Camden-road, N.W.

PAUL, JOHN H., M.D., M.R.C.P. Lond., F.R.C.P. Edin., F.R.C.S. Eng., L.S.A., Med. Superin., Camberwell House Asylum, S.E.

PEART, ROBERT S., M.D., M.R.C.S. Eng., L.S.A., 22, Dockwray-square, North Shields.

PERRY, MARTIN, M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.M., L.S.A., Dep. Coroner for Worcestershire, High-street, Evesham.

PHIPPS, GEORGE C., M.D., L.R.C.P. Edin., L.R.C.S. Edin., L.M., 196, Oxford-road, Manchester.

PIKE, THELWELL, M.D., M.R.C.S. Eng., L.S.A., Weyhill, Andover, Hants.

POCOCK, WILLIAM, M.D., M.R.C.S. Eng., L.S.A., 1, St. John's-villas, Brixton, S.

POLLOCK, A. JULIUS, M.D., M.R.C.P. Lond., M.R.C.S. Eng., Assist. Phys. Charing Cross Hosp., Phys. Foundling Hosp., 21, Montague-place, Russell-square, W.C.

POWELL, JOSIAH T., M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.S.A., 347, City-road, E.C.

POWER, ROBERT F., M.D., M.R.C.P. Lond., F.R.C.S.I., M.R.C.S. Eng., late Phys. Coombe Hosp., 71, Gloucester-place, Portman-square, W.

PRESTON, WILLIAM J., M.D., M.R.C.S. Eng., L.S.A., 41, Belsize-road, St, John's-wood, N.W.

PROCTER, WILLIAM, M.D., M.R.C.S. Eng., L.S.A., F.C.S., Surg. York Disp., 24, Petergate, York

PRYTHERCH, JOHN, M.D., M.R.C.S. Eng., L.S.A., Asst. Phys. Everton Fever Hosp., 145, Netherfield-road North, Liverpool.

PYBURN, JAMES, M.D., L.R.C.P. Lond., M.R.C.S. Eng., L.S.A., 34, Prospect-street, Hull.

RAMSBOTHAM, JOSEPH M., M.D., M.R.C.S. Eng., L.S.A., 15, Amwell-street, E.C.

RANSOM, ROBERT, M.D., F.R.C.S. Eng., L.M., L.S.A., 5, Jesus-lane, Cambridge.

RATTRAY, JOHN, M.D., L.R.C.S. Edin., Shotts Iron Works, Motherwell, Lanarkshire.

REED, G., M.D., M.R.C.S. Eng., L.S.A., Res. Med. Off. Roy. Infirm., Manchester.

RENSHAW, HERBERT S., M.D., L.R.C.P. Lond., L.F.P.S. Glasg., L.M., L.S.A., Salebridge House, Sale, Cheshire.

RICHARDSON, BENJAMIN W., M.A., M.D., F.R.C.P. Lond., F.R.S., 12, Hinde-street, W.

RICHARDSON, CHARLES S., M.D., M.R.C.S. Eng., L.S.A., Dursley, Gloucestershire.

RICHARDSON, EDWARD, M.D., M.R.C.S. Eng., L.S.A., New-road, Commercial-road-east, E.

RICHARDSON, WILLIAM, M.D., A.M. Aberd., M.R.C.P. Lond., 25, Gloucester-gardens, Hyde-park, W.

RICKETTS, CHARLES, M.D., M.R.C.S. Eng., L.S.A., F.G.S., 1, Price-street, Birkenhead.

RING, J., M.D., L.S.A., 47, Clarendon-terrace, Belsize-road, N.W.

ROBERTS, DAVID L., M.D., M.R.C.P. Lond., M.R.C.S. Eng., L.S.A., Surg. St. Mary's Hosp. Manchester, 23, St. John-street, Manchester.

ROBERTS, ANTHONY T., M.D., M.R.C.S. Eng., 73, Lamb's Conduit-street, W.C.

ROBERTS, JOHN, M.D., M.R.C.S. Eng., L.S.A., L.M. Dub., Talarvor, Criccieth, Carnarvon.

ROBERTSON, C. A. LOCKHART, M.D., M.D. Gonville and Caius Coll. Cantab., M.R.C.P. Lond., F.R.C.P. Edin., L.R.C.S. Edin., M.B. Oxon., Med. Supt. Co. Lunatic Asylum, Hayward's-heath, Sussex.

ROBERTSON, WILLIAM, M.D., L.R.C.S. Edin., Surg. R.N., U.S. Club, Edinburgh.

ROBINSON, THOMAS, M.D., M.R.C.S. Eng , L.S.A., 64, Lamb's Conduit-street, W.C.

ROBINSON, EDMUND, M.D., M.R.C.S. Eng., L.S.A., Med. Off. Workh. Inf., Birmingham.

ROBSON, JOHN, M.D., L.S.A., Warrington, Lancashire.

RODEN, WILLIAM, M.D., A.M., L.R.C.P. Edin., F.R.C.S. Eng., L.S.A., J.P., The Grange, Kidderminster.

ROE, JOHN W., M.D., M.R.C.S. Eng., L.M., L.S.A., Coroner for Ellesmere, Dep. Coroner Pimhill Dist., N. Div. Co. Salop, Ellesmere, Salop.

ROGERS, GEORGE, M.D., L.R.C.P. Lond., M.R.C.S. Eng., L.S.A., Longwood House, Long Ashton, Bristol.

ROGERS, THOMAS L., M.D., M.R.C.P. Lond., M.R.C.S. Eng., L.M., Med. Supt. Co. Asyl., Rainhill, Lancashire.

ROGERS, JOSEPH, M.D., M.R.C.S. Eng., L.S.A., 33, Dean-street, W.

ROODS, H. CROWHURST, M.D., M.R.C.S. Eng., Westham, Eastbourne, Sussex.

ROSE, H. COOPER, M.D., M.R.C.S. Eng., L.S.A., F.L.S., F.G.S., Surg. Hampstead Disp., High-street, Hampstead, N.W.

ROSS, GEORGE, M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.S.A., 11, Hart-st., Bloomsbury, W.C.

ROSS, ALEXANDER C., M.D., M.B. Mar. Coll. Aberd., L.R.C.S. Edin., Surg. 92nd Regt.

ROSS, JAMES J., M.D., L.R.C.S. Edin., Med. Att. Northern Infirm., Cons. Phys. Inverness Dist. Asyl., 6, Church-street, Inverness.

ROWEN, FREDERICK J., M.D., M.R.C.S. Eng., L.M. Dub., Annagurra House, Ballylanders, Co. Limerick.

ROYLE, OCTAVIAN N., M.D., F.R.C.S. Eng., Milnthorpe, Westmoreland.

ROYLE, PETER, M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.S.A., J.P., Vernon Lodge, Sale, Manchester.

ROYSTON, CHARLES, M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.M., L.S.A., 1, St. Stephen's-crescent, Westbourne-park, W.

SADLEIR, W., M.D., L.R.C.S.I., L.M., 21, Water-street, Liverpool.

SANDERSON, HUGH J., M.D., M.R.C.P. Lond., M.R.C.S. Eng., L.S.A., Phys. Hosp. for Wom., Soho-sq., 26, Upper Berkeley-street, W.

SANDWITH, HUMPHRY, M.D., F.R.C.P. Lond., Cons. Phys. Hull Gen. Infirm., Beaconsfield, Bucks.

SANKEY, WILLIAM, M.D., M.R.C.S. Eng., L.S.A., Sutton-Valence, Staplehurst, Kent.

SARJANT, JOSIAH, M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.M., West Ferry-road, Isle of Dogs, E.

SAVAGE, THOMAS, M.D., L.R.C.P. Lond., M.R.C.S. Eng., L.S.A., Lect. on Botany, Syd. Coll. Birmingham, Bordesley, Birmingham.

SAVORY, CHARLES T., M.D., M.R.C.S. Eng., L.S.A., 47, Mildmay-pk., Stoke Newington, N.

SAXBY, HENRY L., M.D., Baltasound, Shetland.

SCHOFIELD, FRANK, M.D., M.R.C.S. Eng., L.S.A., Asst. Phys. Camberwell House Lunatic Asylum, S.

SCOTT, WILLIAM, M.D., F.R.C.P. Edin., F.R.C.S. Eng., L.K.Q.C.P. Irel., L.M., Phys. Auchnacloy Fev. Hosp. and Disp. Auchnacloy, Co. Tyrone.

SCULLY, THOMAS, jun., M.D., L.R.C.S.I., L.A.H. Dub., L.M., Clonmel, Co. Tipperary.

SEALY, GEORGE J., M.D., M.R.C.S. Eng., L.S.A., Oatlands-park, Weybridge, Surrey.

SEATON, JOSEPH, M.D., F.R.C.P. Edin., Halliford-house, Sunbury, Middlesex.

SEDGWICK, LEONARD W., M.D., M.R.C.S. Eng., L.S.A., 2, Gloucester-terrace, Hyde-pk. W.

SELWOOD, HENRY C., M.D., M.R.C.S. Eng., L.S.A., Hursley, Winchester.

SEMPLE, R. H., M.D., M.R.C.P. Lond., M.R.C.S. Eng., L.S.A., Phys. Eastern Disp., 8, Torrington-square, W.C.

SEMPLE, A., M.D., L.R.C.S. Edin., Staff Surg., Shorncliffe.

SHARPLEY, THOMAS, M.D., M.R.C.S Eng., L.S.A., County Coroner, Eastgate, Louth, Lincolnshire.

SHAW, WILLIAM, M.D., L.F.P.S. Glasg., L.A.H. Dub., L.M., Lurgan, Co. Armagh.

SHEEN, ALFRED, M.D., M.R.C.S. Eng., 61, Crockherbtown, Cardiff.

SHEPPARD, EDGAR, M.D., M.R.C.P. Lond., F.R.C.S. Eng., L.S.A., Med. Superint. Male Depart. Co. Asyl., Colney-Hatch, Middlesex.

SHETTLE, RICHARD C., M.D., M.R.C.S. Eng., L.S.A., Cann, Shaftesbury, Dorset.

SHORTHOUSE, J. H., M.D., M.R.C.S. Eng., L.S.A., LL.D., The Grove, Carshalton, Surrey.

SKINNER, THOMAS, M.D., L.R.C.S. Edin., L.M., Obst. Phys. Lying-in Hosp., Phys. Female Orph. Asyl. Liverp., 1, St. James-road, Liverpool.

SKRIMSHIRE, GEORGE, M.D., L.S.A., 33, Talbot-terrace, Westbourne-park, W.

SMITH, ALEXANDER M., M.D., M.R.C.S. Edin., Ibroxholm, Govan, Glasgow.

SMITH, W. ABBOTTS, M.D., M.R.C.P. Lond., M.R.C.S. Eng., L.M., L.S.A., Phys. Finsbury Disp., 22, Finsbury-square, E.C.

SMITH, JOSEPH, M.D., M.R.C.S. Eng., L.S.A., Phys. Warrington Disp., Friar's-green House, Warrington.

SMITH, J. SYDNEY, M.D., M.R.C.S. Eng., L.M., L.S.A., Surg. Tiverton Disp., Peter-street, Tiverton, Devon.

SMITH, SAMUEL W., M.D., M.R.C.S. Eng., L.M., L.S.A., Pershore, Worcestershire.

SMITH, T. BROWNING, M.D., West-End, Sittingbourne, Kent.

SOULBY, HENRY, M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.S.A., Waverley House, Hull.

SOUTHWOOD, JOSHUA, M.D., L.R.C.P. Edin., F.R.C.S. Eng., L.S.A., Res. Surg. Metrop. Dis., 9, Fore-street, E.C.

STAMPER, JAMES F., M.D., M.R.C.S. Eng., L.M., L.S.A., Bush-street, Pembroke Dock.

STAPLES, J. H. PROSSER, M.D., M.R.C.S. Eng., 25, Upper Seymour-street west, Connaught-square, W.

STEDMAN, JAMES R., M.D., F.R.C.S. Eng., L.S.A., Surg. Roy. Surrey Co. Hosp., Coroner for Borough, Guildford.

STEWART, DANIEL, M.D., L.R.C.S. Edin., L.M., Liff, Dundee.

STILWELL, ROBERT R., M.D., M.R.C.S. Eng., L.S.A., Beckenham, Kent.

STOCKER, ALONZO H., M.D., M.R.C.P. Lond., M.R.C.S. Eng., L.S.A., Phys. and Superint. Grove Hall Lunatic Asylum, Bow, E.

STOKOE, RICHARD, M.D., F.R.C.S. Eng., L.S.A., Cons. Phys. Peckham and Peckham-rye Disp., Peckham-rye, S.E.

STOVIN, CORNELIUS F., M.D., L.R.C.P. Edin., L.R.C.S., Wheatley, Oxon.

SULLIVAN, EDWARD W., M.D., M.R.C.S. Eng., L.S.A., L.M., Great Ilford, Essex.

SUTHERLAND, JOHN S., M.D., L.R.C.S. Edin., 10, Euston-place, Leamington.

SUTTON, WILLIAM, M.D., M.R.C.S. Eng., L.M., L.S.A., 16, Liverpool-street, Dover.

SWALLOW, J. D., M.D., M.R.C.S. Eng., L.S.A., Surg. St. Martin's Disp., 61, Kennington Park-road, S.E.

TANNER, JOHN, M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.S.A., L.F.P.S. Glasg., L.M., Alfred House, 118, Newington-causeway, S.E.

TANNER, THOMAS H., M.D., M.R.C.P. Lond., M.R.C.S. Eng., F.L.S., F.Z.S., 9, Henrietta-street, Cavendish-square, W.

TAYLER, WILLIAM H., M.D., M.R.C.S. Eng., L.M., L.S.A., Tudor House, Anerley, S.E.

TAYLOR, GEORGE, M.D., F.R.C.S. Eng., L.S.A., Phys. Derbysh. Gen. Infirm., 9, Friargate, Derby.

TAYLOR, CHARLES, M.D., M.R.C.S. Eng., L.S.A. Edin., Ex-Mem. Court of Exam. Apoth. Soc., Pine-house, Camberwell-park, S.

THOMAS, RICHARD R. G., M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.M., Tiverton, Devon.

THOMSON, SPENCER, M.D., L.R.C.S. Edin., L.S.A., Ashton House, Torquay.

TIBBITS, JOHN, M.D., M.R.C.S. Eng., L.M., L.S.A., Jury-street, Warwick.

TIFFEN, ROBERT, M.D., M.R.C.S. Eng., L.S.A., Wigton, Cumberland.

TODD, JOHN M., M.D., M.R.C.S. Eng., L.S.A., Bognor, Sussex.

TORRY, JOHN C., M.D., M.R.C.P. Lond., Phys. Lincoln Co. Hosp. and Gen. Disp., Vis. Phys. Lincoln Lunatic Asylum, Lincoln.

TREWHELLA, HENRY E., M.D., M.R.C.S. Eng., L.M., Headingley, Leeds.

TUKE, THOMAS HARRINGTON, M.D., F.R.C.P. Edin., M.R.C.P. Lond., M.R.C.S. Eng., 37, Albemarle-street, W.

TURNER, GEORGE B., M.D., M.R.C.S. Eng., L.S.A., Surg. East Sussex, Hastings, and St. Leonard's Infirm., 3, Warrior-square, St. Leonard's-on-Sea.

TURNER, ROGER, M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.S.A., Surg. Petworth Gaol, Petworth, Sussex.

TURNOUR, HENRY E., M.D., M.R.C.S. Eng., L.S.A., Surg. Market Rasen Disp., Market Rasen, Lincolnshire.

TURTLE, FREDERICK, M.D., M.R.C.S. Eng., L.S.A., Clifton Lodge, Woodford, Essex.

TYLECOTE, JOHN H., M.D., L.R.C.S. Edin., L.M., L.S.A., Sandon, Stone, Staffordshire.

TYLEY, RICHARD P., M.D., M.R.C.S. Eng., L.S.A., Wedmore, Somerset.

TYTE, EDWARD C., M.D., M.R.C.S. Eng., L.S.A., Harrow-on-the-Hill, Middlesex.

USHER, THOMAS S., M.D., M.R.C.S. Eng., L.S.A., 18, Ocean-place, Aulaby-road, Hull.

VINE, G. J., M.D., F.R.C.S. Eng., L.S.A., 3, Henrietta-street, Covent-garden, W.C.

WALKER, JAMES, M.D., M.R.C.S. Eng., North Frodingham, Driffield, Yorkshire.

WALKER, A., M.D., M.R.C.S. Eng., L.S.A., 21, Langham-place, W.

WALKER, GEORGE C., M.D., M.R.C.S. Eng., L.S.A., Bootle, Liverpool.

WALKER, JOSEPH, M.D., M.R.C.S. Eng., L.D.S., Dent. Surg. and Lect. Westminster Hosp., 22, Grosvenor-street, W.

WARD, JOHN, M.D., M.R.C.S. Eng., 14, Branksome-terrace, Bournemouth, Hants.

WARING, EDWARD J., M.D., M.R.C.P. Lond., F.R.C.S. Eng., F.L.S., 28, George-street, Hanover-square, W.

WARNER, JOHN, M.D., M.R.C.S. Eng., L.M., L.S.A., Weymouth-street, Portland-place, W.

WARWICK, RICHARD A., M.D., M.R.C.S. Eng., 5, Hill-rise, Richmond, Surrey.

WATCHORN, ISAAC, M.D., Raleigh House, Raleigh-road, Nottingham.

WATKINS, EDWIN T., M.D., M.R.C.S. Eng., L.S.A., Surg. St. Pancras Prov. Disp., 61, Guilford-street, W.C.

WATSON, JOHN, M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.M., L.S.A., 6, Southampton-street, Bloomsbury, W.C.

WEBSTER, JOSEPH, M.D., L.R.C.P. Edin., M.R.C.S. Eng., Golcar, Huddersfield.

WEIR, ARCHIBALD, M.D., F.R.C.S. Edin., St. Mungho's, Great Malvern.

WELSH, JAMES, M.D., L.R.C.S. Edin., J.P., Kinghorn, Fifeshire, N.B.

WEST, R. UVEDALE, M.D., L.R.C.P. Edin., F.R.C.S. Edin., L.S.A., Alford, Lincolnshire.

WESTLEY, ROBERT A. W., M.D., M.R.C.S. Eng., L.S.A., 62, Camden-road, N.W.

WHITEHEAD, JOHN, M.D., M.R.C.S. Eng., L.S.A., Ventnor, Isle of Wight.

WHITMARSH, WILLIAM M., M.D., M.R.C.S. Eng., L.M., L.S.A., Dep. Coroner for Wilts, Hounslow, Middlesex.

WHITMORE, JOHN, M.D., M.R.C.S. Eng., L.S.A., Med. Off. Health for St. Marylebone, 15, Wimpole-street, W.

WIGHTMAN, WILLIAM, M.D., M.R.C.S. Eng., L.S.A., Phys. Halifax Infirm. and Gen. Disp., 1, Milton-place, Halifax.

WILKINSON, FREDERIC E., M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.S.A., F.R.G.S., Med. Off. Health Lewisham Dist., Battle-cottage, Sydenham, Kent.

WILLETT, MATTHEW, M.D., M.R.C.S. Eng., Surg. Ace. Bristol Disp., Easton, Bristol.

WILLETT, E. SPARSHALL, M.D., M.R.C.P. Lond., M.R.C.S. Eng., Wyke House, Syon-hill, Isleworth.

WILLIAM, JOHN, M.D., M.R.C.S. Eng., L.S.A., Penygroes, Llanllyfni, Carnarvonshire.

WILLIAMS, C. RICE, M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.S.A., Aberystwith, Cardigan.

WILLIAMS, EDWARD, M.D., M.R.C.S. Eng., L.S.A., Holt-street House, Wrexham.

WILLIAMS, JOHN, M.D., M.R.C.S. Eng., L.S.A., Trosnant Lodge, Pontypool, Monmouth.

WILLIAMS, JOHN, M.D., M.R.C.S. Eng., L.S.A., Swinton, Manchester.

WILLIAMS, S. W. DUCKWORTH, M.D., L.R.C.P. Lond., M.R.C.S. Eng., L.S.A., Asst. Med. Off. Co. Asyl., Hayward's-heath, Sussex.

WILLIAMS, WILLIAM J., M.D., F.F.P.S. Glasg., M.R.C.S. Eng., L.S.A., 231, Oldham-road, Manchester.

WILLIAMS, A. WYNN, M.D., M.R.C.S. Eng., L.S.A., Phys. Samaritan Hosp., Phys. Ace. Western Gen. Disp., 1, Montagu-square, W.

WILLIAMS, W. RHYS, M.D., L.K.Q.C.P. Irel., M.R.C.P. Edin., M.R.C.S. Eng., Bethlehem Hospital, S.

WILLIAMS, DAVID W., M.D., M.R.C.S. Eng., Menai-bridge, Bangor, North Wales.

WILLIAMSON, JAMES, M.D., L.R.C.S. Edin., L.M., L.S.A., 1, Clarendon-villas, Mildmay-park, N.

WILLS, JOHN, M.D., M.R.C.S. Eng., L.S.A., Old Sarum-villas, Castle-street, Salisbury.

WILTSHIRE, ALFRED, M.D., M.R.C.P. Lond., Med. Insp. H.M. Privy Council, Sen. Phys. Islington Disp., 19, Queen Anne-street, Cavendish-square, W.

WISE, WILLIAM C., M.D., L.R.C.P. Lond., M.R.C.S. Eng., L.S.A., Plumstead, Kent.

WITHECOMBE, JOHN R., M.D., M.R.C.P. Lond., F.R.C.S. Eng., L.S.A., 3, Spring-terrace, Richmond, Surrey.

WITHRINGTON, JOHN, M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.M., L.S.A., Paradise-court, Blackburn, Lancashire.

WOOD, HENRY B., M.D., M.R.C.S. Eng., L.S.A., Cranbrook, Kent.

WOOD, JAMES, M.D., L.R.C.P. Edin., L.R.C.S. Edin., Kirby-Overblow, Wetherby, Yorkshire.

WOODMAN, FREDERICK, M.D., L.R.C.P. Lond., M.R.C.S. Eng., Surg. Deal and Walmer Disp., Queen-street, Deal, Kent.

WOODMAN, WILLIAM B., M.D., M.R.C.S. Eng., L.M., M. & L.S.A., Torquay, Devon.

WOODWARD, WILLIAM, M.D., L.R.C.P. Lond., M.R.C.S. Eng., L.M., B.S.A., 22, Foregate-street, Worcester.

WOOLLAM, GEORGE, M.D., L.F.P.S. Glasg., Ashton-under-Lyne.

WOTTON, CHARLES, M.D., M.R.C.S. Eng., L.S.A., King's Langley, Herts.

WRANGHAM, JOHN D., M.D., M.R.C.S. Eng., L.S.A., Wragby, Lincolnshire.

WYMAN, W. S., M.D., M.R.C.S. Eng., L.S.A., Hatfield Broad Oak, Essex.

WYNTER, ANDREW, M.D., M.R.C.P. Lond., 76, Addison-road, Kensington, W.

WYSE, GEORGE, M.D., L.A.H. Dub., L.M., Gov. of Apoth. Hall, Dub., 29, Upper Sackville-street, Dublin.

YEARSLEY, JAMES, M.D., L.R.C.P. Edin., M.R.C.S. Eng., L.S.A., Surg. Ear Infirm., Sackville-st., Surg. Roy. Soc. Musicians, 15, Savile-row, W.

YELD, HENRY J., M.D., M.R.C.S. Eng., L.S.A., L.M. Glasg., Surg. Eye Infirmary, Asst. Surg. Gen. Infirmary, Bridge-street, Sunderland.

YELF, LEONARD K., M.D., M.R.C.S. Eng., L.S.A., Moreton-in-Marsh, Gloucestershire.

YOUNG, HENRY J., M.D., M.R.C.S. Eng., L.M., L.S.A., Bridgnorth, Shropshire.

ASSOCIATES.

COLLINSON, ALFRED, M.D. Aberd., M.R.C.S. Eng., L.S.A., 19, Oxford-terr., Hyde-park, W.

DUNN, ROBERT W., M.R.C.S. Eng., M.S.A., Surg. Farringdon Disp., 13. Surrey-street, Strand, W.C.

FOX, TILBURY, M.D. Lond., M.R.C.P. Lond., Phys. Skin Dep. Univ. Coll. Hosp., 43, Sackville-street, W.

HAVILAND, ALFRED, M.R.C.S. Eng., L.S.A., St. John's Wood, N.W.

HEARNDEN, WILLIAM A., M.D. & Ch.D. Brussels, L.R.C.P. Edin., L.F.P.S. Glasg., L.M., Sutton, Surrey.

KERNOT, GEORGE C., M.D. Alb., M.D. & D.Ph. Giess., L.D.S., L.S.A., Chrisp-street, Poplar.

KERSHAW, JOSEPH, M.R.C.S. Eng., 23, Byrom-street, Manchester.

PALARGUE, J. A., M.R.C.S. Eng., Rochdale-road, Manchester.

PETTINGER, G. W., M.R.C.S. Eng., L.S.A., Sen. Surg. St. Mary's Hosp., 3, Trafford-street, Stretford New-road, Manchester.

REEVE, J. F., M.D. Aberd., M.R.C.S. Eng., L.S.A., 8, Sackville-street, W.

RUNCORN, HENRY, L.S.A., House Surg. St. Mary's Hosp., Manchester.

RUGG, GEORGE PHILIP, M.D. Aberd., L.R.C.P. Lond., M.R.C.S. Eng., L.M., L.S.A., Clapham-road, S.

SEDGWICK, JAMES, M.R.C.S. Eng., L.M., L.S.A., Boroughbridge, Yorkshire.

WHITEHEAD, WALTER, F.R.C.S. Edin., L.F.P.S. Glasg., L.M., L.S.A., Surg. St. Mary's Hosp., 234, Oxford-road, Manchester.

WHITEMAN, R. H., L.R.C.P. Edin., L.S.A., Med. Off. Health, High-street, Putney, Surrey.

HONORARY MEMBERS.

ARGYLL, HIS GRACE THE DUKE OF, K.T., LL.D., F.R.S., Chancellor of the University of St. Andrews, Inverary Castle, Argyllshire.

BELCHER, VICE-ADMIRAL SIR EDWARD, C.B., 22a, Connaught-square, London, W.

BELL, REV. DAVID, M.A., M.D., C.M., Vicar of Goole, Yorkshire.

BELL, OSWALD H., M.D., F.R.C.S., Prof. of Med. Univ. of St. Andrews.

BILLING, ARCHIBALD, M.A., M.D. Oxon, F.R.C.P. Lond., F.R.S., F.G.S., 6, Grosvenor-gate, London, W.

BROWN, REV. WILLIAM, D.D., Prof. of Div. Univ. of St. Andrews, Manse of Gairloch, Dingwall, Ross-shire.

CLARK, THOMAS, M.D., Rector's Assessor, Clyde View, Partick, Glasgow.

COOPER, GEORGE, F.R.C.S. Eng., Master of Apoth. Soc., Mem. Gen. Coun. Med. Educat. and Registr., Brentford, Middlesex.

COPLAND, JAMES, M.D., F.R.C.P. Lond., 50, Old Burlington-street, London.

DAY, GEORGE E., M.A., M.D., F.R.C.P. Lond., F.R.S., Emerit. Prof. of Med. Univ. of St. Andrews, Furzwell-house, Torquay, Devon.

ECKHARD, C., M.D., Prof. of Physiol., Univ. of Giessen.

FARR, WILLIAM, M.D., F.R.S., D.C.L., Gen. Register Office, Somerset-house, London.

FERGUSSON, SIR WILLIAM, Bart., F.R.C.S. Eng., F.R.C.S. Edin., F.R.S., Surg. and Prof. of Surg. King's Coll. Hosp., Serj.-Surgeon to H.M. the Queen, 16, George-street, Hanover-square, London, W.

FROUDE, J. A., M.A., Rector of the University of St. Andrews, 7, Onslow-gardens, London, S.W.

GLAISHER, JAMES, F.R.S., Blackheath, Kent.

MACDONALD, WILLIAM, M.D., F.R.C.P. Edin., F.R.S.E., F.L.S., F.G.S., Prof. of Civ. and Nat. Hist. Univ. of St. Andrews.

MACLAURIN, H. NORMAND, M.A., M.D., Mem. Gen. Coun. Univ. of St. Andrews, H.M.S. "Impregnable," Devonport, Devon.

MITCHELL, W. WEIR, M.D., Wallnut-street, Philadelphia.

OWEN, R., F.R.S., F.R.C.S. Eng., Sheen Lodge, Richmond-park.

PLAYFAIR, LYON, LL.D., F.R.S., C.B., M.P., Prof. of Chem. Univ. of Edin., 14, Abercromby-place, Edinburgh.

POLLI, GIOVANNI, M.D., Milan.

ROBINSON, MR. SERJEANT B. COULSON, 43, Mecklenburgh-square, London.

SARAZIN, JULES, Médecin de Cent Gardes de l'Empéreur, Paris.

SCLATER-BOOTH, GEORGE, M.P., Priory, Odiham, Hants.

STIRLING, SIR WILLIAM MAXWELL, Bart., M.P., LL.D., Keir-house, Dumblane, Perthshire.

STRATHEDEN & CAMPBELL, THE RIGHT HON. LORD, Hartrigge-house, Roxburghshire.

WATSON, SIR THOMAS, Bart., M.D., LL.D. Cantab., D.C.L. Oxon, F.R.C.P. Lond., F.K.Q.C.P. Irel., F.R.S., 16, Henrietta-street, Cavendish-square, London.

LAWS

OF THE

ST. ANDREWS MEDICAL GRADUATES' ASSOCIATION.

TITLE.

1.—The Association shall be called "The St. Andrews Medical Graduates' Association."

OBJECTS.

2.—The objects of the Association shall be the advancement of the Science and Art of Medicine, and of General Science and Literature, the maintenance of the interests of the Medical Graduates of the University, and the cultivation of social intercourse and good fellowship.

CONSTITUTION.

3.—The Association shall consist of Members, Honorary Members, and Associates.

4.—All Medical Graduates of the University of St. Andrews shall be eligible as Members, if recommended by two Members of the Association.

5.—All Members of the General Council, all Professors, and all non-medical Graduates of the University of St. Andrews, shall be eligible as Honorary Members, as well as such other learned and scientific men as may be recommended by the Council.

6.—All legally qualified Medical Practitioners shall be eligible for admission as Associates.

7.—Members, Honorary Members, and Associates, shall be admitted only at the General Sessions of the Association. The election shall be by ballot, and no one shall be declared elected unless two-thirds of the Members present vote in his favour.

8.—A Member, Honorary Member, or Associate, may withdraw from the Association by paying such subscriptions as may be due from him, and signifying his intention in writing to the President.

9.—No Member, Honorary Member, or Associate, shall be removed from the Association except in accordance with the following regulations. A written notice of the proposed removal, signed by two Members of the

Association, shall be sent to the Honorary Secretary, who shall immediately forward a copy of the charge to the Member accused, and shall at the same time summon the Council to meet within twenty-one days. He shall send a notice of the subject to be discussed to each Member of the Council at least fourteen days before the date of such meeting. If the Council shall resolve by a majority of those present, that the Member so accused ought to be expelled, a notice shall be forthwith sent to each Member of the Association, making the next General Session special for the consideration of such removal, and if two-thirds of the Members voting shall be of opinion that the Member in question shall be expelled, the President shall direct the Honorary Secretary to remove his name from the list of Members. The votes shall be taken by ballot.

10.—The subscription constituting a Member or Associate shall be Five Shillings annually, due on the first of January in each year.

EXECUTIVE.

11.—The Officers of the Association shall be elected from the Members, and shall consist of a President, Six Vice-Presidents, a Treasurer, a Secretary, and a Council of Thirty-two; in whom the power of framing bye-laws, and of directing the affairs of the Association, shall be vested.

12.—Five Members of the Council shall form a quorum.

13.—The Officers of the Association shall be elected by ballot at each Anniversary Session of the Association.

14.—The Officers of the Association shall be eligible for re-election, except that two of the Vice-Presidents and eight of the Council shall retire every year.

15.—The business of the President shall be to preside at the Sessions of the Association, and at the Meetings of the Council; in his absence one of the Vice-Presidents, or the Treasurer, or any Member of the Council chosen by the Members present, shall take the chair.

16.—The Treasurer, or some person appointed by him, shall receive all moneys due to the Association.

17.—The money in the hands of the Treasurer, which shall not be immediately required for the uses of the Association, shall be vested in such speedily available securities as shall be approved of by the Council.

18.—The Council shall lay before the Members, at each Anniversary Session, a report of their proceedings during the past year, and also an account of the receipts and expenditure of the Association.

19.—The Council shall meet at least once in two months, unless by special resolution to the contrary.

20.—The annual accounts of the receipts and expenditure of the Association shall be audited by a Committee of three Members selected at the preceding Anniversary Session from among the Members at large.

21.—The Secretary shall have the management of the general correspondence of the Association, and of such other business as may arise in carrying out its objects.

SESSIONS.

22.—The Association shall hold an Anniversary Session, commencing on St. Andrew's day, or on such other day as the Council may determine. The place of such Session, its duration, and the business to be transacted, shall be arranged by the Council.

23.—The Members and their friends shall hold an Anniversary Dinner on the last day of each Anniversary Session, at such place and time as the Council may determine; the President for the year shall be in the chair.

24.—No alteration in the Laws of the Association shall be made, except at a General Session. Notice of the alteration to be proposed must also have been laid before the Council at least a month previously.

25.—The Council shall have power to call a General Session of the Members at any time, and shall also be required to do so within one month, upon receiving a requisition in writing to that effect from not less than twenty Members of the Association.

26.—All Special General Sessions of the Association shall be held at such place as the Council may appoint.

GENERAL.

27.—The Council shall have power to publish the proceedings of the Association, and to make such charge for them as they may deem right.

28.—The Council shall have power to order the name of any Member whose subscription is two years in arrear to be removed from the list of Members.

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GENERAL SESSION.

April 8.

The Session was held at the Rooms of the Medical Society of London, 32a, George Street, Hanover Square.

The minutes of the last Session were read and confirmed.

Dr. Nesfield, Manchester; Dr. Crompton, Manchester; Dr. Candy, 109th Infantry; Dr. Richardson, Tunbridge Wells; Dr. Richardson, Commercial Road, London; Dr. Lloyd, London; Dr. Maugham, Carnarvon; Dr. Badcock, Brighton; Dr. Cockerton, Montgomery; Dr. Walker, Bootle; Dr. Dyer, Ringwood; Dr. Pocock, Brixton; Dr. Marsden, London; Dr. Collins, Wolverhampton; Dr. Whitmarsh, Hounslow; Dr. Yeld, Sunderland; Dr. Boulton, Beverley; Dr. Berwick, Sunderland; Dr. Jeffery, Eastbourne; Dr. Torry, Lincoln; Dr. Whitehead, Ventnor; Dr. Morgan, Bath; Dr. Harrison, Lincoln; Dr. Fegan, Charlton; Dr. Savage, Bordesley; Dr. Hughes, Liverpool; Dr. Jennings, Eastry; Dr. Cotton, London; Dr. Barras, Govan; Dr. Hodgson, Northallerton; Dr. Fisher, Lytham; Dr. Hearder, Carmarthen; Dr. Beaman, London; Dr. Palmer, Ormskirk; Dr. D. Moore, Hastings; Dr. Goddard, London; Dr. Tayler, Anerley; Dr. Yelf, Moreton-in-Marsh; Dr. May, Exeter; Dr. Cross, Liverpool; Dr. Croucher, St. Leonards-on-Sea; Dr. Hecksher, Manchester; Dr. Willett, Isleworth; Dr. Miller, Sidmouth; Dr. Robson, Warrington; Dr. Young, 'Bridgenorth; Dr. Tylecote, Sandon; Dr. Barlow, Manchester; Dr. Brown, Burgess Hill; Dr. Henry, London; Dr. Currey, Lismore; Dr. Sandwith, Beaconsfield; Dr. Fleming, Omagh; Dr. Roberts, Talarvor; Dr. Griffiths, Portmadoc; Dr. Farquharson, Tillicoultry; Dr. Stedman, Guildford; Dr. Edwards, Sparkbrook; Dr. Cheeseman, Buckingham; Dr. Kersey, Littlebourne; Dr. Adamson, St. Andrews; Dr. Henderson, Leith; Dr. Staples, London; Dr. Lyell, Dundee; Dr. Lawrence, Montrose; Dr. Adam, Colney Hatch; Dr. Barwise, Bury; Dr. Finch, Blackheath; Dr. Parker, Bermondsey; Dr. Welsh, Lochcarron; Dr. Duncan, Lochalsh; Dr. Hayward, Leeds; Dr. Robinson, Birmingham; Dr. Taylor, Camberwell; Dr. Meller, Hunstanton, St. Edmunds; Dr. Ford, Chulmleigh; Dr. Osborn, London; Dr. Lockhart Robertson, Haywards Heath; Dr. Blumer, Monkwear-

mouth; Dr. Mackintosh, Glasgow; Dr. Martin, Pendlebury; Dr. Rowen, Denton; Dr. Cheetham, Islington; Dr. Harland, Mayfield; Dr. Westley, London; Dr. Boycott, London; Dr. Williams, Menai Bridge; Dr. Power, London; Dr. Sealey, Oatlands Park; Dr. Goodall Jones, Liverpool; Dr. Wyman, Hatfield Broad Oak; Dr. Ransom, Cambridge; Dr. Turtle, Woodford; Dr. Cory, London; Dr. Murray, Oaken; Dr. Giles, Hastings; Dr. Goldsmith, Worthing; Dr. Horner, Chester; and Dr. Barratt, Brighton,—were elected Members.

G. W. Pettinger, Esq., Manchester; H. Runcorn, Esq., Manchester; J. Kershaw, Esq., Manchester; J. A. Palargue, Esq., Manchester; Dr. Hearnden, Sutton; Dr. Rugg, Clapham Road; and Dr. Reeve, London,—were elected Associates.

Lord Stratheden and Campbell; Sir W. Stirling Maxwell, Bart., of Keir, M.P.; Sir Thomas Watson, Bart., F.R.S., London; Sir W. Ferguson, Bart., F.R.S., London; Vice-Admiral Sir Edward Belcher, C.B., London; G. Sclater-Booth, Esq., M.P., Odiham Priory; Rev. W. Bell, M.D., Goole; Dr. Copland, London; Dr. W. Farr, F.R.S., London; J. Glaisher, Esq., F.R.S., Blackheath; Mr. Serjeant Robinson, London; G. Cooper, Esq., Brentford; Dr. Weir Mitchell, Philadelphia; Dr. Polli, Milan; M. Jules Sarazin, Paris; and Herr Eckhard, Giessen,—were elected Honorary Members.

Dr. Paul of Camberwell proposed, and Dr. O'Connor of London seconded,—

> "That the President be requested to offer the condolence and sympathy of the Association to the friends of the late Drs. Thiselton Dyer, Ray, and Fayrer." Carried unanimously.

The Honorary Secretary read a Report on Parliamentary Matters, which is published in the Appendix of Vol. I. of the Transactions.

Dr. Day Goss of London proposed, and Dr. Beverley Bogg of Haulbowline, seconded,—

> "That Dr. Sedgwick's Report be received and approved." Carried unanimously.

Dr. Cholmeley of London proposed, and Dr. Ross of London seconded,—

> "That the Honorary Secretary be directed to draw a statement showing the grounds on which the Doctors of Medicine of St. Andrews claim a right to be placed on the General Council of the University, and refuting the arguments raised

against their admission; and that he be also directed to forward such statement to the Governing Bodies of the University, with an expression of the deep regret of this Association at the determination of the General Council to oppose the claim of the Doctors of Medicine, and of the hope that this opposition will not be persevered in." Carried unanimously.

A memorandum was accordingly drawn by the Honorary Secretary, and is published in the Appendix of Vol. I. of the Transactions.

The President brought forward a recommendation of Dr. Turtle of Woodford,—

"That the Council be requested to consider whether the Association could bring out at the beginning of each year 'An Annual of Medicine,' not to exceed 250 pages, and composed of a tersely written abstract of the more notable discoveries and additions to Medical Science during the previous year."

The subject was remitted to the Council for consideration at their next meeting, when the President was requested to thank Dr. Turtle for his suggestion, and to express the regret of the Council that they were not able at present to entertain it.

The President having explained his views on the subject of the Registration of Disease, Dr. Cholmeley of London proposed, and Dr. Royston of London seconded,—

"That the President of the Poor Law Board be requested to receive the President and other Members of the Association as a deputation on this matter." Carried unanimously.

Dr. Paul of Camberwell, proposed, and Dr. Greenhalgh of London seconded,—

"That the thanks of this Association be given to the Medical Journals for the hearty and efficient support rendered to it since its formation." Carried unanimously.

The Honorary Secretary having announced that he was indebted to his friend, Augustus Edward Browne, Esq., of Clifton Chambers, Lincoln's Inn Fields, for the design for a seal which the Council had done him the honour to accept, it was unanimously resolved—

"That the thanks of the Association be given to Augustus Edward Browne, Esq., for his very beautiful design."

GENERAL SESSION.

July 20 and 21.

The Session was held at the Freemasons' Tavern, Great Queen Street, Lincoln's Inn Fields.

The minutes of the last Session were read and confirmed.

Dr. Tayleur Gwynne, Whitchurch; Dr. Merryweather, Sheffield; Dr. Thelwell Pike, Weyhill; Dr. Fleetwood Buckle, H.M.S. Victory; Dr. Vine, London; Dr. Semple, London; Dr. Bainbridge, London; Dr. Aldred, Bayswater; Dr. Dow, Bayswater; Dr. and Surgeon-Major Huish, Hounslow; Dr. Sadleir, Scallaheen; Dr. Barry, Twickenham; Dr. Daldy, London: Dr. Swallow, Kennington; Dr. Madge, London; Dr. Jepson, Stone; Dr. Kernot, Poplar; Dr. and Dep.-Insp.-Gen. Gordon, Portsmouth; Dr. and Staff-Surgeon Andrew Semple; Dr. Blackmore, Salisbury; and Dr. Ring, Kilburn,—were elected Members.

Dr. Tilbury Fox, London; and Dr. G. C. Kernot, Poplar,—were elected Associates.

Dr. Billing, London, was elected an Honorary Member.

A letter from Alfred Haviland, Esq., recommending the establishment of a School of State Medicine was referred to the Council for consideration.

The Honorary Secretary read the Report of the Council.

Dr. Greenhalgh of London proposed, and Dr. Nicholls of Devizes seconded,—

> "That the Report and Resolutions of the Council be approved and adopted." Carried *nemine contradicente*.

The following day, July 21st, arrangements were made for members to visit the Tomb of Harvey, at Hempstead, and on the way there, by the kind courtesy of the Right Honble. Lord Braybrooke, his House and Grounds of Audley End.

The following resolution of the Council was reported.

"That the Council recommend those Members of the Association who are Governors of the Royal Medical Benevolent College, at Epsom, to support for the election to a Foundation Scholarship, Charles William Beckett, the son of a brother Graduate."

ANNIVERSARY SESSION.

December 2 and 3.

The Second Anniversary Session of the Association was held at Willis's Rooms, St. James's, commencing on Wednesday, December 3rd.

The minutes of the previous Session were read and confirmed.

Dr. Reed, Manchester; Dr. Foote, Sunderland; Dr. Campbell, Stourbridge; Dr. Black, Chesterfield; Dr. Holman, Reigate; Dr. Lush, M.P., Salisbury; Dr. John Mackenzie, Aldershot; Dr. Helsham, Brixton; Dr. Podmore Jones, Liverpool; and Dr. Walker, London,—were elected Members.

W. Whitehead, Esq., Manchester; and R. H. Whiteman, Esq. Putney,—were elected Associates.

Professor Owen, F.R.S., London; Dr. W. Farr, F.R.S., London; and J. A. Froude, Esq., Rector of the University of St. Andrews,—were elected Honorary Members.

The Officers for 1869 were elected.

Dr. Ballard, Dr. Cooper Rose, and Dr. Rhys Williams, were elected Auditors for the ensuing year.

The Honorary Secretary, on the part of the Council, moved the addition of the following words to Law IV., "if recommended by two Members of the Association;" and the following new Law;—

"28. The Council shall have power to order the name of any Member whose subscription is two years in arrear to be removed from the list of Members." Carried unanimously.

The Treasurer's Report was read.

The Honorary Secretary read the Report of the Council.

Dr. Pike of Weyhill proposed, and Dr. Watson of London seconded,—

"That the Report of the Council be received and adopted." Carried unanimously.

Dr. Crisp of London proposed, and Dr. Drysdale of London seconded,—

"That the Council be instructed to take such steps as they may deem most advisable to obtain the repeal of the regulation limiting the number of non-resident Graduates in Medicine to ten annually." Carried unanimously.

The Honorary Secretary read a Report on the Parasitic Theory of Disease.

Dr. Tuke read a Report on the Criminal Responsibility of the Insane.

Dr. Crisp read a Paper on the Influence of a Moist Atmosphere in the production of Phthisis.

Dr. Lloyd Roberts read a Paper on Imperforate Anus.

Dr. Maund read a Paper on Diphtheritic Paralysis.

The other Papers were not read from want of time.

The President delivered the Annual Address to a large audience, composed not only of members of the profession but of the general public, ladies and gentlemen.

The subject was "The World of Physic and the World."

Vice-Admiral Sir Edward Belcher, C.B., proposed, and Dr. Cholmeley seconded, a cordial vote of thanks to the President for his eloquent and forcible address. Carried unanimously.

Dr. Copland trusted that it would be published separately for general circulation.

The President returned thanks, and presented to Dr. Sedgwick, the Honorary Secretary, in the name of the Members, a carved oak writing table and a silver inkstand, together with an illuminated address which was read by Dr. Edwards Crisp.

Dr. Sedgwick returned his hearty thanks.

The Anniversary Dinner was held in the evening. The President in the chair. Vice-Admiral Sir E. Belcher, C.B., Dr. Lush, M.P., Rev. Dr. Bell, Professor Owen, F.R.S., Dr. W. Farr, F.R S., and Dr. Gardiner Hill, honoured the Association with their presence as guests. Sir E. Belcher, Captain Thompson, and Dr. Cooper Rose, returned thanks for the Army, Navy, and Volunteers. Dr. Lush, M.P., returned thanks for the Houses of Parliament. The President proposed, "The University of St. Andrews;"

Dr. Tuke proposed, "Science, Literature, and Art," to which Professor Owen responded; Dr. Farr proposed, "The President and the St. Andrews Medical Graduates' Association;" Dr. Day proposed, "The Press, Medical and General," to which Mr. Brudenell Carter and Mr. Barnett responded; and Dr. Cooper Rose proposed, "The Officers and Council of the Association," to which Dr. Greenhalgh responded.

I.

BUSINESS OF THE ASSOCIATION AND COMMUNICATIONS IN CONNECTION THEREWITH.

REPORT OF THE COUNCIL.

April 8, 1868.

The first great object for which the St. Andrews Medical Graduates' Association was founded is achieved.

The Doctors of Medicine of St. Andrews have obtained the franchise, and more than this, have obtained, as was logically their right, a seat in the General Council of their University. The accomplishment of these two important results, while it marks in the most signal manner the power which attends combined and brotherly action for a common good, has not completed the work of the Association. Questions of the greatest importance to the University and the public yet remain, and the need of united action is now only the greater.

At the present time one chief and pressing duty devolves on the Medical Graduates of the Scottish Universities: the selection of a Member to represent them and their fellow-graduates in Parliament.

This duty and the wish to give a vote in common with their brethren, has induced many members of the Association to apply to the Council for advice and guidance in this matter.

Fully recognising the responsibility which attaches to the course to which it is urged, your Council have felt it a duty to meet you with some distinct and definite expression of opinion.

First, as to the qualifications required of the Member of Parliament for the conjoined Universities.

It cannot be doubted that in future Parliaments social and sanitary questions will assume an importance to which they have not hitherto reached, and that it will be for the advantage of the nation that on these and kindred topics the voice of the whole profession, or of some large section of it, shall be authoritatively heard. That this may be, there must be Members who are actual practitioners of Medicine, and the Medical Member must be returned by a constituency wholly or in great part medical, and must be sent to Parliament for this special and distinct purpose. If he be returned by an ordinary constituency from motives of ordinary party politics he

cannot be accepted as the spokesman of Medicine. If this be so, then it is clearly the duty of the Medical Graduates of Edinburgh and St. Andrews to strive to elect such a Member, and it is equally clearly the duty of this Association not to fritter away its strength in isolated action, but by combination and concentration to endeavour as far as in it lies to have for its representative in the Commons House of Parliament a Doctor of Medicine of position and authority. Deeply impressed with this duty your Council at their last meeting passed the following resolutions :—

1. "That it is desirable that the representative in Parliament of the conjoined Universities of Edinburgh and St. Andrews should be a *bonâ fide* member of the medical profession."
2. "That this Council recommends the Association to support Dr. Richardson as a candidate for the representation in Parliament of the conjoined Universities of Edinburgh and St. Andrews."

Your Council would strongly urge upon you the necessity of voting unanimously, of sinking minor differences for the good of the whole body, and of relegating party politics to their proper sphere in boroughs and counties.

B. W. Richardson, M.D., F.R.S., *President.*
Leonard W. Sedgwick, M.D., *Hon. Sec.*

REPORT OF THE COUNCIL.

December 2, 1868.

The Second Anniversary Session of the St. Andrew's Medical Graduates' Association takes place under circumstances which are the best vindication of the existence of such a Society.

While your Council had to meet the members last year with an account of just claims unrecognised, of the great bulk of the Graduates of St. Andrews without a voice in the government of the University, or a vote for its Member in Parliament, they have this year the gratification of congratulating you on the possession of both these valued privileges.

One has just been exercised, the Parliamentary Franchise; and while your Council cannot conceal their deep regret that a Doctor of Medicine has not been selected, and that the late period to which the announcement of Dr. Richardson's candidature was necessarily delayed rendered his success impossible, they look with confidence to the next election for a general recognition of the principles advocated in their last report, and they firmly trust that a constituency, mainly medical, will then be represented in the House of Commons, by a Doctor of Medicine of one or other of the conjoined Universities.

The other franchise, the right to vote in the General Council of the University confers on the Medical Graduates a power they have never before possessed.

Your Council ask your earnest attention to the regulation which prohibits the granting of the Degree of Doctor of Medicine to a greater number of non-resident Graduates than ten in each year. This your Council feel to be an injustice alike to the University and to the general body of the Profession. An injustice to the University, because in time the Medical Faculty will thus be starved almost out of existence; and an injustice to the Profession, because a competent candidate is thus unable to obtain the honour which he has earned whenever there are ten other candidates equally entitled to it. In no other University is the character of the Degree maintained by a

limitation in the number of its recipients, and your Council urge on you the importance of endeavouring to obtain the removal of such an injurious exception in the case of St. Andrews. The alteration rests with the University Court, subject to the sanction of the Chancellor and the consent of the Queen in Council, and there can be no doubt that a proper representation to these authorities of the injustice of the regulation will result in a change beneficial to the University, the Profession, and the Public.

The numbers of the Association steadily increase; 130 new members, 9 associates, and 20 honorary members have been elected since last Anniversary Session, making the whole number who have joined up to the present, 532 members, 15 associates, and 28 honorary members; of whom 4 members have resigned, and 5 have died. This year your Council lament the loss of one Vice-President, Dr. Thiselton Dyer of London; two Members of Council, Dr. Fayrer of Henley-in-Arden, and Dr. Ray of Dulwich; and one other member of the Association, Dr. Stookes of Liverpool. Their colleagues will cherish the memory of earnest and high-minded brethren.

The finances of the Association are satisfactory, and your Council hope to be able to continue the Annual Volume of Transactions without any increase of the present very small subscription.

Some alterations of the laws, which your Council believe will conduce to the advantage of the Association, will be submitted to you.

In the interests of the University of St. Andrews and its Medical Graduates your Council only ask for a continuance of the same earnest energy and the same brotherly union which has already achieved so much.

B. W. Richardson, M.D., F.R.S., *President.*
Leonard W. Sedgwick, M.D., *Hon. Sec.*

REPORT OF THE TREASURER.

The Account of Receipts and Expenditure for the year 1867 has been audited, and a balance of £5 10s. 3d. in favour of the Association is shown to exist. An Abstract is appended.

When the expenditure of the present year can be accurately ascertained it will probably be found to be about £230, including five hundred copies of the first volume of the Transactions and the cost of the Anniversary Dinner. This amount will be rather more than covered by the Annual Subscriptions and the sale of Dinner Tickets.

J. H. PAUL, M.D.,
Hon. Treasurer.

December 2, 1868.

Abstract of Accounts for 1867.

	£	s.	d.		£	s.	d.
Subscriptions . .	94	0	0	Stationery & Stamps	28	4	4
Donation . . .	1	0	0	Printing	13	9	6
Dinner Tickets . .	70	7	0	Advertisements .	11	15	6
				Sundries	7	16	5
				Dinner	98	11	0
				Balance	5	10	3
	£165	7	0		£165	7	0

Examined and found correct,

H. Mortimer Rowdon, M.D.
J. T. Griffith, M.D. } *Auditors.*

J. H. Paul, M.D., *Hon. Treasurer.*
L. W. Sedgwick, M.D., *Hon. Sec.*

ON THE LIMITATION OF MEDICAL DEGREES TO TEN ANNUALLY.

Dr. Crisp (London) said he had very much pleasure in proposing the resolution that had been entrusted to him—

> "That the Council be instructed to take such steps as they may deem most advisable to obtain the repeal of the regulation limiting the number of non-resident Graduates in Medicine to ten annually."

No good reason, he said, could be assigned, if the profession was to be saddled with twenty licensing bodies, why the University of St. Andrews should be shorn of the number of its Graduates, but potent reasons might be given why that University should not be crippled in the manner recommended by the Commissioners of 1858. The fact was, that the question was made one of pounds, shillings, and pence, for the benefit of the other Scotch Universities, and the public weal and the good of the profession were not considered. The University had enabled many men in active practice, whose knowledge of disease was obtained at the bedside, not merely from books, to take the Doctor's Degree, and as long as the examination was of a searching and practical character such a privilege should not be withdrawn. There was one great advantage that the University of St. Andrews had over all other examining boards of this country, except that of the University of London—namely, that the examiners were not the teachers of those they examined; a matter, he thought, of great importance. Mr. Lowe, in his recent address at the University of London after his election, said—"There is another cause to which the high position of this University (London) is owing: the wisdom of its founders has provided that the Senate who appoint the Examiners shall not be allowed themselves to take part in the examination, and thus you have avoided the rock upon which many medical institutions have split—that the body to confer the degree jobs in its own examina-

tions, and conducts them when they would be much better conducted by others."

But there was another matter connected with this subject that he, Dr. Crisp, could not forbear alluding to—namely, the qualifications of the Graduates. Nothing generally was gained by pulling down others and endeavouring to exalt themselves, but as very odious comparisons had been made, and as some of the authorities of the University of Edinburgh had taken a very prominent part in opposition to the obtaining of the franchise by the Medical Graduates of St. Andrews, he might properly quote the following statistics, which would have been used in the Houses of Parliament had any further opposition been offered. He found by an analysis that he had made of the qualifications of the 498 graduates whose names appeared in the first volume of the Transactions that their united diplomas, exclusive of 85 Licentiates of Midwifery, amounted to 1469 in number; averaging, with the Midwifery Degree, more than three diplomas to each Graduate; a number exceeding that of the diplomas of the Graduates of any other University in the United Kingdom. He might add that more than three hundred of the Edinburgh Medical Graduates had only the Degree of their own University. He concluded by saying that the Graduates had the matter now largely in their own hands, and he urged them not to rest satisfied as long as this stigma attached to them; their motto should be *Excelsior*. He believed that he was expressing the sentiments of all present when he said that they asked for a searching examination not inferior to any in the United Kingdom, but they asked also that this unjust restriction should be removed.

Dr. Charles Drysdale, in seconding the motion, said that he considered that the narrow spirit in which most examining bodies in this country insisted upon residence in their own peculiar locality, was one of the greatest hindrances to the advancement of learning at present.

There was much truth in the analogy taken from the stamping of the precious metals. Pure gold and silver received the impress of the Mint, whether they came from this country, from Australia, or from California, and circulated on the same terms. And although there was no doubt that there was considerably more difficulty in testing the mental acquirements of the candidate for a Degree in Medicine or Arts, still he had not a shadow of hesitation in asserting that a residential qualification, at least, was utterly valueless as a test of proficiency. It had been argued with a certain degree

of truth, that mere examinations, such as those boldly proposed by Mr. Simon in his recent address, would prove valueless, because some persons would cram for these examinations, and as soon as they had passed the ordeal, would forget their hastily acquired knowledge as quickly as they had learnt it. He was not quite prepared to grant the truth of this objection, since he could easily conceive of examinations being made so practical by clinical questions as to prevent such cramming; but, at any rate, there could be nothing urged against the system of examination followed at St. Andrews, where the production of certificates of attendance for several years at hospitals and medical schools was required. His own cherished scheme was, that the State should appoint a set of examiners, to examine for Degrees, in London, Dublin, Edinburgh, Glasgow, St. Andrews, Liverpool, Manchester, &c., &c., all who had conformed to the requirements for a Degree in Medicine. Such a Degree could be granted for a small sum, say £10, and should alone be accepted as entitling its possessor to hold any medical post in the army, navy, or poor law service. After taking this unexceptionable Degree, persons might add any other ornamental one they pleased. Persons practising privately might, perhaps, not be required to pass this state Degree, although many held that they ought, before being allowed to take any other medical diploma. Perhaps the maxim of "Caveat emptor" was applicable, however, to the latter case. Until this desirable end was attained, he warmly seconded the proposal to keep up the only truly liberal Degree in this country. And, as the majority of the profession was in their favour, he doubted not that such a resolution would soon be carried into effect. The way in which medical examining bodies wasted the funds of the public at present was disgraceful to our character as a practical nation.

Dr. Cleveland supported the resolution, and suggested that the advocacy of a definite plan in reference to the admission of medical graduates would be better than simply asking for the removal of the present restriction.

The President observed that there could be no reasonable argument on the present basis or premise against the admission of any number of candidates for the Degree. If ten might be admitted each year, the selection being based on no special qualification, except age, why should not eleven be admitted, or why should not the limit stop at nine? He had thought at first that perhaps some calculation had been made in respect to the number of those practitioners who re-

quired the Degree from year to year; but if this were the case the calculation was utterly at fault, as no one year could be compared with another, and as, in no year, would there be so few as ten applicants from the whole of the united kingdom, the colonies, and the army, and navy. He (the President) would place the number of aspirants at least four times above the number now allowed, even on the terms that none under forty should be admitted for examination. He was himself being constantly applied to for advice how to proceed, by gentlemen who wished to present themselves to the examiners. He held it to be a necessity to allow an increase in the number of candidates, and urged that there was sufficient cause for the necessity. It could not be concealed that the profession of medicine came from the middle class of society, and that every year a certain number of most useful practical members would always be admitted, who, by force of circumstances, had been obliged to forego in their education those advantages which were supposed to belong to a career in an university. But was it fair, therefore, to exclude these gentlemen from the Degree of M.D. if they were ready to undergo, at a free university, a rigid and extensive examination. He thought not; he was open to say that the present graduates of the University of St. Andrews who had obtained their Degrees under the free principle were not inferior in position to the graduates of any other of the universities. Why should they be? Their education, though out of an university, was virtually the same, and if the examination were the same, in what particulars could there be a difference? The President then urged the question of re-introducing the open principle on the ground of its great public utility. A man who had commenced his medical life in general practice, who had gained repute in his locality, and who felt he could throw off the drudgery of incessant work, was surely entitled to the privilege of offering himself for examination for the higher Degree without having to submit to the forms demanded of the student, forms after all not necessary to the acquirement of accurate knowledge. Well, if a man so disposed did make himself ready, by the means of learning which were at hand, to pass a fair and proper examination, what but good could come from the effort? The man was raised in social status by an effort that must make him a more accomplished practitioner. His previous experience in practice was improved by many months of labour, spent in order to get up the new subjects in medical science which had been advanced since his student days, and his mind, so apt in the daily routine to become

torpid, was requickened by the ordeal to which he had surrendered himself. On these grounds he (the President) should support the general principle conveyed in the motion. The principle was sound, just, liberal. On the present basis, on the plan of admitting but ten graduates a year without residence, the University of St. Andrews, as a medical university, would practically be dead, when she might be living, as she ought to live, doing an efficient service to physic, which no other university could do, adding to her reputation and her usefulness in the world, and extending and consolidating her influence, through her graduates, in all parts of the empire.

Dr. Seaton supported the resolution.

Dr. Sedgwick said that the St. Andrews Medical Graduates' Association was bound by the terms of its bond to the "maintenance of the interests of the Medical Graduates of the University." The interests were those of the members of a liberal and learned profession, which could be served by no narrow cramping restrictions. It was right that the Doctors of Medicine should be a select body, but select by reason of their acquirements, not by reason of the fewness of their number. It was right, too, that the Degree should not be had merely for the asking, but a man's title to it should be his knowledge, as tested by a sound and searching examination, and not requiring as an essential condition of validity that no more than nine other candidates should have, at the same time, proved their fitness for the honour. The five hundred medical graduates who formed the Association had, by the strength of their arguments and by their unity of action, obtained for themselves and their brethren, the university and, by consequence, the parliamentary franchise. This matter was equally in their own hands, and the same united action would secure the removal of a regulation as false in principle as it was injurious in practice.

Dr. Wynn Williams quite agreed with Dr. Crisp in considering the present arrangement as manifestly unjust, not only to the University, but more especially to a large body of earnest working men, who, having a legal qualification enabling them to practise their profession, have not only carried on with often very great success the practical part of their profession, but have kept up and increased their more scientific knowledge, keeping pace with their more fortunate brethren, whose means or other circumstances enabled them to continue to study their profession in the hospitals. St. Andrews was the University where, until the late arrangement,

the hardworking medical man was enabled to take his Degree in medicine—a Degree often so much coveted by the successful general practitioner who desired to enter, so to speak, the higher, more lucrative, and less arduous branch of his profession. Some fresh arrangement should be made which would enable a man who wished it, to present himself for examination for the Degree of M.D. at St. Andrews, without residence or restriction as to numbers. Let the examination be as strict and severe as the authorities choose to make it; but as long as a man holding a legal qualification to practise, is enabled to pass it, he ought to be allowed to do so without being obliged to leave for a twelvemonth or more that profitable and useful sphere which he has attained at so much labour and cost, to find when he returns, his place to a certain extent occupied by another, and he himself obliged to begin as it were *de novo*. Dr. Williams thought that some such arrangement as this should be made, namely, that when a man had attained a certain age, and had been a given number of years in the practice of his profession, he should be allowed to present himself for examination. This would, of course, in no way interfere with the usual curriculum of the student; but he did not think, without some set-off of years and practice against study at the hospitals and schools, that the object the meeting is evidently so unanimous in desiring to attain, will be granted. Personally, he was of opinion that as long as a man had acquired a scientific and practical knowledge of his profession, and was able to satisfy the examiners, after a searching examination, that such was the case, he ought to be granted his degree without being asked any questions as to when or where he acquired his knowledge.

Dr. H. Day heartily concurred in the purpose intended to be accomplished by Dr. Crisp's resolution, for he could not think that there should be any limitation in the number of medical graduates admitted annually, supposing that the examination they underwent was sufficiently stringent in character; and, as he concluded that the present examination of the ten graduates admitted was a fair test of their ability, he could not see why, if good for that number, it should not be equally good for ten times that number. Furthermore, he observed, that as the University of London granted its Degrees to non-resident candidates, and as the examination they underwent was admitted to be the one only needful test of their fitness for the Degree, no limit being put to the number who might present themselves for examination, he held it to be unjust to place

any restriction upon the number who might obtain the Degree of an university having any claim to the antiquity and celebrity which St. Andrews possessed.

Dr. Thelwell Pike also supported the resolution. He urged that there should be no restriction whatever on those admitted to the examination for M.D.; that whoever desired to present himself should be allowed to do so, no matter how, when, or where he obtained his knowledge. The only essential conditions were that the examinations should be stringent and extended, and the examiners men of tried worth.

Dr. Crisp having replied, the resolution was carried unanimously.

REPORT ON PARLIAMENTARY MATTERS.

BY

LEONARD W. SEDGWICK, M.D., HON. SEC. OF THE ASSOCIATION.

To complete the narrative of the efforts made by the Association to obtain for the Medical Graduates of St. Andrews their admission to the General Council of the University, and a vote for its Member in Parliament, little need be added to the account given in the first volume of the Transactions.

The Government having recognised the justice of the claim of the Doctors of Medicine to the University and the Parliamentary Franchise, did not recede from their position, and in neither House of Parliament was there a sound of opposition to their proposals. That part of the Reform Bill for Scotland which granted the franchise to all the Medical Graduates was passed without opposition. The clauses having special reference to this matter, stand in the Act as follows :—

"27. The Chancellor, the Members of the University Court, and the Professors for the Time being of each of the Universities of Scotland, and also every Person whose name is for the time being on the Register, made up in Terms of the Provisions hereinafter set forth, of the General Council of such University, shall, if of full age, and not subject to any legal Incapacity, be entitled to vote in the election of a member to serve in any future Parliament for such University in Terms of this Act."

"28. Under the Conditions as to Registration hereinafter mentioned, the following Persons shall be Members of General Council of the respective Universities :—

"All Persons on whom the University to which such General Council belongs has, after Examination, conferred the Degree of Doctor of Medicine, or Doctor of Science, or Bachelor of Divinity, or Bachelor of Laws, or Bachelor of Medicine, or Bachelor of Science, or any other Degree that may be hereafter instituted."

The Registrar of each University is to keep a Registration Book in which is to be entered the name, designation, qualification, and place of residence of every person qualified to be a member of General Council.

Into this Registration Book, within two months after the passing of the Act, the names of all members of General Council in the existing Register were to be transferred; and on payment to the general University Fund of a registration fee of twenty shillings, the name of every qualified person applying for registration, from time to time, is also to be entered. The fee of twenty shillings is a life payment, no further annual or other payment being required. From this Registration Book an alphabetical Register of Members of Council is to be made up annually. This year the Register is to contain the names of those only who were entered in the Registration Book before the first day of October; but in future the Registrar is to proceed to prepare on the first day of December in each year, "a new alphabetical Register for the Year to commence on the First Day of January next ensuing, which new Register he shall make up by transferring to it from that in force at the Time the Names, Designations, and Addresses (with such corrections as he may consider necessary) of all Members not known to be dead, and by transferring to it from the Registration Book the Names, Designations, Qualifications, and ordinary Places of Residence of all Persons who shall have paid the Registration Fee since the Day of commencing to make up the Register of the preceding Year, and who are not known to have died since making Payment." This Register, after revision by the Registrar and two Assistant-Registrars, is to be authenticated by the signature of the Vice-Chancellor, on or before the Thirty-first of December, and remains in force as conclusive proof of the right of persons to be Members of the General Council for the year ensuing. The Registration Book and the Register are to be open for inspection in the office of the Registrar at all reasonable times, and copies may be made on payment of a fee of one shilling for every hundred names, or fractional part thereof, copied.

Any Member of General Council can appeal against the insertion of the name of any person whom he considers not to be qualified, during the first ten days of January in each year.

The person appealed against is to have immediate notice, and the appeal is to be heard by the University Court between the twentieth and the thirtieth day of the same month. The judgment of the

Court is final, and the President has power, if necessary, to alter the Register in accordance with the decision.

Any one who believes that his name has been improperly omitted can appeal in like manner, and the President of the University Court can order it to be inserted if such be the judgment of the Court after hearing the appeal.

In the elections for Members of Parliament the votes may be taken by voting papers, signed in the presence of a Justice of the Peace, and containing the name of one or more Members of Council, whom the voter empowers to deliver the voting paper at the poll.

Such, in brief, are the provisions of the Magna Charta of the Doctors of Medicine of St. Andrews, achieved not less by unity of action than by strength of position.

PRESENTATION OF A TESTIMONIAL TO DR. SEDGWICK.

The President, at the close of his acknowledgment of the vote of thanks for his address, said:—I have now to perform one of the most pleasing duties that could fall to me as President of this large and influential Association. We all know that the existence of every society depends on the activity and the intelligence of the officers; we know further that amongst the officers there is one all-important officer upon whose hands the real, the daily work of the organization rests—I mean the Honorary Secretary. Well, it happens in our Association that we are more than usually fortunate in our Secretary. From the first day since our Association was fully constituted until this day Dr. Sedgwick (loud cheers) has laboured for us with a zeal, judgment, perseverance, and conscientiousness which I have never known surpassed (cheers). You, Fellow Graduates, who have been working with the Council for twelve months past, in the hard-fought and well-won battle which we have waged in support of our rights, our claim to have a voice in the election of the Member who shall represent our University in the House of Commons, you, I say, know that not a little work—not a little anxiety had to be undertaken by every one who was interested. But I assure you that had you been as well acquainted with all the workers as I have been, you would have been astounded at the vast extra labour which has been carried on in connection with this effort by Dr. Sedgwick. By night and by day he has been engaged in our service, never doubting and never relaxing a moment until the victory was gained. Moreover, industry is not the only good quality of our friend. We esteem him equally for his excellent judgment, his courteous attention, and his quick desire to study, in every way, our interests, and, I may even say, our happiness (cheers). On previous occasions we have publicly thanked Dr. Sedgwick for these his exertions; but on the present occasion it has been generally felt that something more than mere formal

thanks are demanded. The Association has, therefore, committed to me the task of presenting him with a simple, but, we trust, an appropriate testimonial, viz., a carved oak writing table, a silver inkstand, and an illuminated address. Dr. Sedgwick, in the name of the St. Andrews Medical Graduates' Association, I present you with this earnest but admittedly inadequate recognition of the esteem and gratitude of the Members of the Association, together with the expression of our united hope that you may long live and labour amongst us, our friend and our brother. I now call on Dr. Crisp, the Secretary to the Testimonial Committee, to read the Address.

Dr. Crisp said that it gave him very great pleasure to know that, last year, he had suggested the presentation of a testimonial to Dr. Sedgwick, and that, this year, Dr. Cordwent and himself had been enabled to obtain, in a very short time. the means of giving some practical evidence of their gratitude to their Honorary Secretary. He then read the Address, which was as follows :—

"The Members of the St. Andrews Medical Graduates' Association, at this, their second Anniversary Meeting, have the pleasure to present to Leonard W. Sedgwick, M.D., their Honorary Secretary, a writing table and a silver inkstand, as a trifling testimonial and recognition of the laborious and lasting services he has performed for them. The Graduates feel that not only the success of the Association has been very greatly promoted by Dr. Sedgwick's zeal, devotion, and excellent judgment in conducting the duties of his office — duties for many months past unusually onerous—but that his courteous and considerate manner has contributed essentially to the good feeling and unanimity which has up to this time signally characterised the progress of the Association. The Graduates trust that Dr. Sedgwick will accept this testimonial as an individual as well as a collective expression of their gratitude and esteem."

Dr. Sedgwick, in reply, said :—Brother Graduates, I know not how to tell you of the pleasure which this your handsome present has given me. It is a pleasure made up of many pleasures— that of having so worked for the common good as to have obtained the esteem of my Brother Graduates—that of having this evidence of your regard presented to me by our distinguished President—and that of your giving me the testimonial in the face of so large and important an assembly. Do not, I pray you, measure my gratitude

by the number of my words. They must be few, for I cannot thank you as I would wish. But I do thank you, and from my heart. I thank my friends, Drs. Crisp and Cordwent, for the great trouble they have taken in the matter. I thank you all for your good will and your kindly feeling. And I thank the President not only for counsel and assistance in time of need, but for the warm terms in which he has expressed his friendship for me to-day. I do not offer you these thanks, Brother Graduates, as mere lip-service, rather would I prove their genuineness by still working for our Association and for our old University.

II.

COMMUNICATIONS ON MEDICAL AND SCIENTIFIC SUBJECTS.

ON THE WORLD OF PHYSIC AND THE WORLD.

By Benjamin W. Richardson, M.A., M.D., F.R.S.,
President.

Fellow Graduates, Ladies and Gentlemen,

A decision of the Council of the St. Andrews Medical Graduates' Association, that members should invite friends—ladies and gentlemen—to the Annual Address, has imposed on me a duty difficult as novel. Bound to speak in the name of a body of scholars in physic who are striving to hold an advanced position in their day and generation, I am excluded dealing with any one of those refined subjects in practical medical science, which could give scope for possible display of the learning of the profession in its own particular walk, but which could not, at the same time, be made of interest to a general assembly. I have tried to meet the difficulty by selecting a subject that shall be common to all scholars and all interests; I mean, the mutual relationships of the communion of physic and the community at large;—"*The World of Physic and the World.*"

Did you ever, Ladies and Gentlemen, let your minds have range so as to take in, in one grasp of view, the world of physic as an isolation from other worlds of life on this planet? Have you thought of it distinctively, as you have of the nations of the earth, the languages, the races? Perchance you have not; and if you have not, then may the picture be worthy your regard, even though, with indifferent pencil, it be put before you in bare outline.

We then, who, according to our several gifts, minister to the physical ills of mankind, form, the world over, an effective strength of probably one million of men, and, excluding the significant old ones of both sexes, say twenty women. In every second of the passing time a detachment of from ten to fifteen of us is ministering to the sick. Each minute brings one of us at least in, or near, the presence of death; each minute brings one of us at least in presence

of the first breath of the living temple; making us the eye witnesses of the natural fact so immortally expressed, that in the grand scheme of Creation, "death has no sting, the grave no victory." In the midst of these first and last phenomena of human existence, we live, conversant also at other times with endless changes of pain and pleasure, sorrow and happiness, strength and weakness, lying between the first and last.

As we are represented in English and Welsh life, a fair representation, I think, of a general kind, there are nine of us, on the average, equipped and ready for the service of ten thousand living people: in this ten thousand, four hundred and forty-four are constantly sick, of whom two hundred and twenty-two die in the year; each member of the world of physic has therefore an average of forty-seven claimants for his skill constantly on hand, *i.e.* nearly two every hour, on the average of time, by night and by day.

Our million, as the result of this persistency and necessity of action, knows little rest; and as men, like all things else of the earth subjected to motion, exist a longer or a shorter time according to wear and tear, so we, of all sections of the living human world who are above manual labour, and indeed of the community at large, up to fifty-five years of age, present the shortest of existences, in the forms by which, during our ephemera, we are known and recognized. At twenty-five years of age, we die at the rate of nearly five to four of the general community; at forty-five, as two to one and four-fifths; then, indeed, those who can live through so much, hold good their ground, and, at fifty-five, are a shade better than the average; at sixty-five, a shade worse, and after that better.

From the certain average destiny that is before our million, and from its close converse at all times with extremes of anxiety, it might be inferred that the life is one of gloom. It is not so. As men, in the positive face of danger, become, even against their natures, brave, by a process of reasoning peculiar to the occasion and extorted from them by virtue of necessity, so medical men, in the actual presence of the most serious evils, as evils are commonly understood, become, if the term be not a paradox, implacably resigned to anxiety, and live in it apart from it, save as all men live in it when it comes to them, or theirs, individually. This adaptation of the mind to the absolute necessity of time and circumstance is, in the case of the representatives of the world of physic, an inestimable blessing to all. The mind, too anxious about results, the hand, too tender for action, the voice, too hesitant to suggest or to

command, is not the mind, the hand, the voice, to be in action when the issues of life, or of death stand, in array. So our mistress Nature ordains, not by bending of her laws to the men, but by the bending of the men to her laws, that they shall be, as they are, common sharers with other men in joy and in sorrow. Men, who live in the presence of more sorrow than the rest of the world, but who, by hourly converse with that sorrow, are mentally lifted from it. Men, knowing, scientifically, how best to remove or appease sorrow, and who are ready ever to accept this task as a part of their daily duty, by virtue of an acquired nature which is in them, but not originally of them, and which, were it of them, would prohibit the exercise of the duties by which they are made useful to the world, and to themselves as a part of the world.

The million of physic, as to its origin, is, and always has been, and probably always will be, from the class of mankind, middle in station. The rich and ennobled, can never be sufficiently numerous to yield the million demanded; the poor, can never find the means to educate the million. It is true that in this country a noble duke, the Duke of Richmond, once stood on the roll of the Royal College of Physicians, and it is equally true that a carpenter, John Hunter, came from the work-bench to bring the Royal College of Surgeons into fame, and himself into unspoken, because unspeakable, honour. But these are grand exceptions, alone even in England, elsewhere unknown. Here again Nature bends humanity to her law. It is essential that men whose daily duties extend alike to all, to the monarch, or president of the state, as to the lowest of the low, should be accessible to all, and by mind, by heart, by will, the helpmates of all; and our million represents, in this sense, more distinctly than any other living world, the common brotherhood of the world. To us, in our actual vocation, in our dealing with abstract man, blood is blood, muscle is muscle, nerve is nerve, brain is brain, eye is eye, tooth is tooth, pain is pain, exhaustion is exhaustion, apart from all the other considerations by which other men, or by which we ourselves out of our special vocation, exalt or degrade the human social life. In moments of exalted ambition we may lament our lot, but we must bow to it as to the inevitable, and, in moments of solemn thought, we must accept it as blessed. So is it received indeed by the world; the peasant rests upon it, feeling, that in the hands of the faithful healer, he is as safe as the king; the proudest, the most absolute of rulers, a ruler of many rulers, trusting in it, begs his physician to treat the sharer of his crown

even as he would treat the simplest of his subjects; while Christendom, speaking by the mouths of its holiest of the holy, loves, as if in the most carefully chosen and expressive words, to illustrate the character of its Divine Founder by his deeds as the Physician.

From the very universality of the working life of our world of physic its power is indefinite, better, perhaps, say indefinable. The power of the priest is not ours; the power of the lawgiver is not ours; the power of the soldier is not ours; we have never in our history, as a class, been connected with national revolutions, national conquests, national crises of any kind, for good or evil. Esculapius, according to the ancient way of expressing his greatness, was received into the number of the gods, simply because he cultivated the art of physic with a little more subtlety than his fellows. And, William Harvey, the nearest personal friend, as the physician, of any crowned king, left no more powerful nor any lesser work than the discovery of the circulation of the blood.

And yet, negative though we appear to have been, and appear to be, we have at all times, and in all countries, exerted a veiled influence, which, like all great natural forces, has been the more potent for that it has not been ostentatiously exposed to public view. When great catastrophies of disease have shaken the hearts of men, the world of physic has ever been at hand, a firm and reliable power, keen to investigate, bold to intervene, and, by natural contempt of danger, qualified to sustain hope and prevent the panic of despair. In our daily routine it is a part of our accepted duty to upraise the downcast; while we hold up to the world, by the constant strain of our efforts and ambition to improve our science in all its parts, the indisputable proof that we are prepared, to any extent, to lessen the necessity which calls us into action, and brings into our hands the labour on which we depend for our daily bread. Thus, the world of physic is in many ways a present power; a power silent but sure; a passive power, inoffensive yet propulsive; acting solely by its units, yet strong because its units, a million times multiplied, have a common intention and will.

To this direct power of the world of physic, past and present, a power honestly claimed by every honest member of that world, may be added certain indirect influences which have not been without their value. Medicine has, at all times, produced men who have cultivated pure physical science with an energy and perseverance shown by no other special class of mankind. Our Kepler laid

the first steps in the study of vision, and with these, the first steps in optics as a part of physical science. Our Mahow first suggested the compound character of the air, and the existence in it of a something that sustained the combustion of flame, and the combustion of the living body. Our Boerhaave excited a taste for chemistry and gave a character to that science, which since his day have never declined. Our Black determined the product of burning carbon, and showed that product as exhaled from the ordinary furnace and the living lung. Our Young propounded the now accepted theory of light as undulation. Our Hunter remodelled the study of natural history, and laid the foundation of that study of the history of nature, past and present, to which Cuvier, who in exile practised surgery, devoted his best life, and which the still living master, Owen, also of the same profession, has so splendidly advanced. Our Hartley expounded the fact of the communication of motion in vibrations, from the outer universe into the animal body. Our Locke made logic a true science, and gave meaning to the study of the human understanding itself. Our Erasmus Darwin wrote the earliest accurate readings of the living relationships between plants and animals, and bequeathed the study which his great descendant and namesake, who still is with us, has so marvellously elaborated. Our Linnæus and De Candolle vie with each other as founders of that philosophical study and classification of plants, which the distinguished Hooker, with a zeal worthy of his masters, in this day maintains. Our Metcalfe, with labour of learning not often seen, was amongst the first who introduced the theory of the unity of force in nature. Our Haller conferred precision and breadth of expression on the literature of science; and our Goldsmith introduced into works of fiction a purity, beauty, and chasteness which, before his time, were unknown. Our Mungo Parke was the traveller who made the first decided progress in discovering the interior of Africa and the sources of the Nile; and our noble Livingstone, whom we hope soon to see amongst us again, has completed what his less fortunate brother, even at the sacrifice of his life, left unachieved.

In naming these simple matters of history, shall I say too much if I claim, that in the advance of the physical sciences, the world of physic has taken, and sustained, a pre-eminence which those who have made pure physics their sole study, have not surpassed.

If we turn from physical to social science, we find again, in the world of physic the same pervading influence. The teaching of the

deaf and dumb and blind to hold converse with their more fortunate fellows, has been the work of our zealous progressionists. The foundations of sanitary science were laid by Southwood Smith and his army of medical followers at home and abroad. From the galled limbs of the insane the iron fetters were boldly and for ever struck off by one of our brethren, who, this day, is even here amongst us, Gardiner Hill; while by the labours, profound as beautiful, of Conolly, the humane management of the insane was not only consolidated, but the whole subject was reduced to an order, simplicity, and advancement, which contemporary narrative could not possibly reveal in all its meaning, plenitude and beneficence.

Connected with this same subject, the world of physic, in silent measure, is increasingly labouring to discover the intimate relationship of crime and disease, the end of which work must be to demonstrate to the legislator, that the absolute theory of punishment, as the one and universal remedy for crime, is equivalent to crime itself in folly and in evil; and that the major part of the miserables who occupy the felon's dock or cell, are specimens of mentally deformed humanity, to save whom, or the similar of them, from disfiguring another age, we must go back to the principles of their growth, training, development, and cast improvement there.

Lastly, in its estimates of life,—of life as, a something that may be calculated upon, even in the money market, insured upon, realized, as of so much gross earthly value, changing in value according to habit, condition, descent, labour,—of the comparative life values of professions and trades, of sexes, of towns, counties, nations,—our medical world has produced learning which has no rival. One man of the medical world, who to-day also honours us with his presence, has, in this direction, invented a science, the ultimate of value, which cannot be foreseen, it is so wonderful. You have anticipated me when I name Dr. William Farr.

The learning of the world of physic, its own learning, is, at first sight, singularly irregular and diverse. In this advanced Europe, in America, and in the other civilized nations, the physician is, one might think, another order of mortal, when compared, with his brother of the far east, untutored and rudely skilled. In this civilized land, see the learned professional what aids he calls? He enters the chemical laboratory, and brings forth potent agents with which he moves the living organism almost as he wills; now he puts it into dead slumber and perfect rest; anon, he increases its

muscular action till the will is subservient to the stronger power. He subjects it to air of extreme heat, and he freezes parts of it until the structure is like stone. If he lists, he puts his ear to the body and hears its mechanical throbbings and breathings, and reduces what he hears to such perfect rule that he can calculate on changes of structure and function, as distinctly as if he could see the hidden workings. If he lists, he takes up his reflecting glass, peers straight into the eye and other parts of the organism, and observes the changes of structure and function with exactitude. If he lists, he takes out his delicate thermometer, and measuring the animal force, determines, with refined nicety, the balance of overaction or inaction. If he lists, he counts the pulsation of the body with a stop watch, or weighs the pulse, I may say, with his sphygmographic scale, and makes it write for him its own history, black upon white, or white upon black. He takes out of the body a speck of its blood or other structure, and, with delicate lens, reads off the history of great physical changes going on in the whole or part of the body, and, it may be, forecasts results, with true discernment. In short, he gathers such absolute information respecting the body and its conditions, that if he were master to rule, as he is to observe, its phenomena, he were master of all that man could achieve in physic, and would be now that perfect animal engineer he one day must become.

In uncivilized, or partly civilized, Thibet, see, on the other hand, the so-styled learned physician, and the aids he calls. In the month of September, when the day breaks over his magnificent mountains, watch this man leaving his Lamasery to collect his remedies. A leathern bag and a teakettle, carry all his wants. Armed with pointed iron-capped staff and hook, like a Druid of our own old time, he marches forth with his train of pupils, and roaming the mountains, picks out of the laboratory of Nature his medicinal stores, from branch, from shrub, from root. With the declining sun he returns, laden with his spoils, next day culls them, dries them in the air, packs them, labels them, stores them in some safe garner of the quiet Lamasery, and, in his honest soul, believes that the wealth of the whole medical world is in his safe keeping. Called to the couch of the sick or the dying, he is content to hear of pain, to read off signs of oppression, and, striking his fingers across the pulse of each wrist, as a musician doth the strings of his instrument, he is satisfied. The phenomena he sees are with him easily understood; they are the assaults of a demon

who must be expelled. So many diseases, so many demons, and, let it not be doubted, so many remedies. From the wonderful pouch by the side of that physician, come forth those dried plants he gathered on the mountain side, and down the throat of the afflicted certain of them go, in nauseous powder. Or, should the remedy not be in the pouch, oh! wonderful Lama physician, with more than homœopathic skill, he writes the name of the remedy on a scrap of paper, moistens the paper with his lips, rolls it into a pill, and administers it to the faithful; who, straightway swallowing, with the earnest belief that the name is as good as the thing, when it comes through proper hands, believes and lives, or believes and dies, as the case may be.

But before the last event shall happen, be the patient rich enough to bear the operation, our good Lama has one or two other resources at hand, belonging to the imaginative; which resources are bold, and, in proportion as they are bold, effective. By that most convenient of theories, that every disease is a demon within the man, the good Lama has a hold to which we civilized have no claim. Between the actual existence of a thing, and firm faith in the existence, whether it be or not, the gulf is narrow in all minds, absent in most; and so, the Tartar patient is, to his physician, as good as a man who should have veritably of veritably a demon within him. Well, I put to you here, to all, what would you wish for most if you believed as firmly that you had a demon in your tooth, making it ache, as that you had a tooth to be made to ache? I suspect you would like to have that demon cleared out. Further, if you were a Lama physician, and knew the quality of the demon, and his best mode of exit, you would, I think, attempt to remove him. Our Lama sympathizes. He says to his patient, "I can get rid of this demon by certain magical prayers, but you, being a wealthy man, are afflicted with a very proud demon, in fact, quite a swell demon, and he will not go away unless you find him a thorough good horse to carry him off." And so the horse is brought out, properly accoutred, the prayers are recited, and then, the demon getting inside the horse, and the physician outside, they go away together, and unless the demon leaves the horse, or the physician disposes of both, demon and physician remain as intimate as is proper so long as the horse lives. Where the demon goes afterwards I cannot say, I suppose to his native place.

In our world of physic, the Lama and the civilized physician are at the antipodes of science, for which reason I have put them in

contrast, leaving you to fill up the intervening castes if you like the labour.*

The diversities of the learning of the world of physic, great as they are on many subjects, do not separate it as a body. Skilled or rude, its representatives, by virtue of their daily work, have many qualities, and ripened faculties, in common. Brought into close converse and intimate communion with living humanity, at times when it is feeblest and most distressed, the physician sees, as in a mirror, the back as well as the front of human nature, and, instinctively, recognizes its shape and character with an imposed accuracy of observation, often extremely painful. The inner life, the relation of the soul to the body : the vehement will, built on the powerful heart: the vehement will, succumbing under the breakdown of the heart once powerful: the brain, nobly balanced in structure and in function : the brain unbalanced : the criminal, by nature criminal, strong in untruth, vaulting even into temptation, proud of his own selfishness, dishonest by intent: the virtuous, by nature virtuous, born virtuous, constructed on physical mould of virtue, steadfast in al trials, generous in all conflicts, patient in all afflictions, truthful in all temptations, honest in all emergencies: the body and soul together balanced, well or ill : the body exceeding the soul, the soul exceeding the body. All these conditions come before the physician, and, *nolens volens*, must be read.

The world at large, busied with other concerns, and not, understanding this forced study of the world of medicine, often criticises medical men for giving opinions respecting the mental conditions, particularly of criminals, contrary to the opinions which revenge, unmoderated by knowledge, insensibly inspires. I picture before me a powerful man, for years the esteemed of the most estimable. What of him ? His abashed friends tell me he has sinned. I see him, and his broken intermitting useless heart, proclaims to me an irresponsible being clothed still in all the apparent majesty of reason. I know his brain is like a flickering gas-light when the pressure is unequal at the main. But for me to say that that man is irresponsible, were to subject myself to ridicule, or even to punishment. Conscious of his own helplessness, uncertain, shrinking from himself and the world, what wonder that the man, in brief time, should be discovered a suicide ?

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* See, in relation to this description of the Lama Physician, the fascinating book, "Travels in Tartary, Thibet, and China," by M. Huc.

I have put a common experience, and because, it is necessary, I have tried to put it forcibly, from nature. All the judges on the bench could not, by all the legal knowledge in every book of law, understand correctly a case of this kind. That man, brought when he sinned, before a judge, would probably have received a moral discourse on the iniquity of crime, as well as sentence to punishment for offence against the State. Learned judge, exalted above thy fellows, and mostly wise, but now mocking Heaven, thou wert as well occupied in lecturing a rabid dog on the sin of biting, or, in punishing a cobra di capello, for the crime of carrying deadly poison!

It is the forced reading of the inner life, of the mutual dependencies of the soul and body, of the phenomena of outward action in man as dependent on inward function, and of formed character, as based on organic construction, that makes medical men appear to the world so special in their views in favour of mercy and against extreme punishment. On these points, the members of the profession are not weaker, nor better, nor more generous than the rest of the world; they are, simply, more informed; a good century in advance of the world at the present rate of education; so advanced indeed, that it is hardly possible to announce what they know, lest it seem extravagant and dangerous. But their knowledge charges them, irresistibly, with mercy. As they cannot, if they would, be angry with a man whose muscles are paralysed, because he declines to take up his bed and walk; neither can they be vindictive in regard to a man whose brain, physically unbalanced, impels action which, by the abstract standard of right and wrong, *is* wrong. We take a pigeon: we freeze a segment of its brain, and straightway, from destruction of balance of power, the creature begins to walk backwards, turn summersaults, and do other strange things. Shall we be angry with the bird, because it presents phenomena which are not, according to our views, natural? In brief, the world of medicine sees the phenomena of human action, in form and character, even as it sees the human body, itself a mere phenomenon. It beholds the mental form every day, as though in a stereoscope, solid, defined: it may have aversions to what it sees, or likings; but it cannot, in face of reason, anathematize or praise, as if the objects seen were deformed or beautiful according to their own absolute sovereignty over themselves.

These views lead the world, in general, to recognize what it calls the active sympathy and benevolence of the world medical. The truth spoken, this is an error: medical men are not more sympathetic,

nor more benevolent, than the rest of mankind: the qualities, so recognized, are the results of a better knowledge of the strength and weakness of the animal organism, and of the dependence of mental manifestations on corporeal health.

Such, in poor sketch, is the world of medicine. A world of a million; dying earlier in life than the other millions; living in daily converse with pain and sorrow, yet not sorrowful; a middle-class million, stretching out its hands to work for lowliest or highest born; a million, having little direct power in the nation, yet giving birth to new action which becomes, by development, powerful, and inspires nobility of thought and life; a million, differently and diversely learned; a million, having by necessity of daily practice, the deepest of insights into the inner life; a million, brought to the love of mercy by the bare knowledge of the weakness of mankind which calls for mercy.

THE WORLD AT LARGE AND THE WORLD OF PHYSIC.

The relationships of the world at large with the world of physic are, on the whole. good. Three things I notice specially on this head. First, that in its distresses, the world at large never fails to run to the world of physic; second, that by its returns for services rendered, the general world keeps the physic world well provided for; and thirdly, that although the general world does not pay to the world of physic any enthusiastic admiration, it pays, usually, sincere respect, after all probably the safest reward. These foundations are sound and satisfactory: but there are, nevertheless, certain differences between the two worlds, which it were well if possible to remove, and which it cannot but be useful candidly to consider.

The great, perhaps I should say, the one, serious difference is, that the world at large holds the practical knowledge of physic to be uncertain and limited. Hence, in its dire distress, it is wont to make little distinction between the most conscientious and learned of practitioners, and the meanest of pretenders in medicine: thus it supports and sustains what we call quackeries, respecting which act we, wisely or unwisely, are keenly sensitive. On both sides there is error. There is error on the part of the world, in that it expects more of us than, in the time allotted to human learning, we can acquire and bring into practice; there is error in that it does not,

by learning, for itself, something about the laws of life, become conversant with the characteristics of the conscientious physician as distinct from those of the charlatan; and there is error in that it does not distinguish between positive knowledge and mere opinion or dogma. There is error again, on the part of the world of physic, in that it does not look firmly enough into the causes of its own weakness; does not, sufficiently, take into account the natural fears and feelings of mankind; does not sufficiently strive to make the world partakers with it in its knowledge, in its weakness, in its power; but rather bends to the prejudices of the world than attempts to remove them by demonstration of their mischief and of its own unwavering, however slow, progress towards what is right.

What mutual understandings then should subsist between the world of physic and the world?

The man representing the world at large should, I think, first consider what, in the fullest sense, he can expect the man of physic to perform for him, and how far Nature herself ordains that the success of the man of physic shall be permitted. In this sense he, the man of the world, should make the utmost allowance for the tremendous odds which the medical man has to contend with, by reason of the inevitable law of Nature, that the continuance of life is based on the continuance of death. If we save the life of an individual never so many times, a time will come when we shall be utterly unable to save that life. So art at last must fail. Or, to proceed to a more general proposition: so inexorable is Nature in maintaining the balance of life and death, that she sometimes strikes down portions of mankind wholesale, by causes which she does not give us time to study, until the event has swept by us: she apportions mortalities to seasons, and diseases to races: she puts on earth, for her own purposes, beings so differently constituted that they defy the application of any one absolute rule of treatment: she stamps character and build by hereditary transmission, and brings us face to face with special differences which we cannot possibly control. These rules, set by Nature herself, enforce limitations on the skill of the physician which are primary and should be, at once, conceded as inevitable. Physicians would be gods should they be other than they are, units of Nature, contending with Nature altogether, contending always, often losing, and never winning actually, but sometimes seeming to win when to win is in accord with the great argument of natural necessity.

The representative of the world at large should remember, again, that the man of physic, having in all cases to deal with free will in man, must needs be perpetually crossed in his finest knowledge by the fruits of free will. If we could raise the dead, there are men who, so raised, would die again as quickly as they could. We do not as yet raise the dead, but it is our constant duty to put men into such mode of life that they shall be raised from the misery of sickness into comparative health and comfort; after which, we find, as constantly, they go back against their reason to their old ways, and are the same as ever. I think I may say it for all my learned brethren, present and to come, that there is not the remotest expectation this difficulty will ever be removed: for, turning to our own selves, we find that we, by example of free will, are the most determined specimens of it. We are retail and wholesale specimens at one and the same time: by force of habit, we run into special dangers from which other men instinctively shrink; and, although we are conscious of practising a profession which will make us have the shortest life value of the community, we stick to it as if it were the elixir vitæ itself and something more.

A man is not engaged in the practice of medicine one twelvemonth before he learns the fact of this resistance of free will. Some medical men grow hopeless thenceforth, and follow their art, feeling that, whatever they do, it is so much the same, that if they only use the knowledge they have gained it is as good as any new knowledge, and that for them to endeavour to discover new truths, were indeed a task pitiably bootless. Others more enthusiastic, make straight ahead, always determining to do, in spite of the more than dead opposition which so constantly meets them. But even these, after the days of youthful inspiration have passed, sigh as they labour. They believed in the beauty of life, in the glory of saving life, in the admiration of mankind for the duty. A dream! They wake to the consciousness that of all things least appreciated, life is that thing. They see riding on the heart of every adult, man or woman, the disease *atra cura*, "black care," worry, which sits deadly, and for which there is no physical remedy less than death. Or as one of our fellow graduates and members, a true poet, Dr. Black, plaintively but too truly sings

"The heart when sickened by despair
"The loss of hope and light of prayer,
"Hath but another step to brave
"The path of darkness to the grave."

They see men, without any obvious reason, running tilt against each other, and dogging each other, and persecuting each other, all for mere dross, unless it be that collision of mind is necessary for existence of mind: they see the affections of the young wounded, and the parental soul stabbed till it is lifeless: they see men ready, at all times and ages, to sacrifice any length of life to any shortness of ambition. Now a line of youths in their prime is in view, with fathers, mothers, sisters, and hosts of friends cheering them. They are going to row a race. If that act did, of necessity, deprive those youths of half an eye, or half a nose, or even half a finger, they would not be induced to enter the lists so readily, neither would they be cheered so vociferously; but because it costs them half a life, on they go: for, what is life? Again, there is a line of older men, some very old, as we have seen recently, with boisterous friends, wives, sons, and daughters, this time cheering them on, and noisome enemies hissing in discord. These men are running to row in the boat of the State; they spend a year's accumulated animal force in shouting to a crowd that will not listen, but shouts again; and they get, or get not, the famous two letters to their names by giving up a good tenth of all that remains of their ephemera.

I refer to these things not in slight of reasonable competitive effort, physical or mental. I, who in the enthusiasm of scientific research have never hesitated to accept what the pusilanimous would call personal risk, have, least of all, a right to uphold fear of death or fear of shortening life, as a virtue. I do nothing of that sort, but look on the little general regard paid by humanity to its own individual existence as one of the most perfect of natural ordinances. I think no more noble words were ever spoken than those of brave old Sir Humfrey Gilbert, "Give me leave therefore, without offence, always to live and die in this mind: that he is not worthy to live at all, that for fear or danger of life shunneth his country's service and his own honour, seeing that death is inevitable and the fame of virtue immortal." I think that our professional enemy, free will, is never so useful as when it is sustaining the brave. The truth that

"All men think all men mortal but themselves,"

is, to me, a great truth. I see free will setting the world proudly and properly above the most skilled of curers. I would not ask even for a miracle to save myself from death, at the loss of

free will; but I point out, none the less earnestly, that the world can never have those perfect results of medical science it craves for, while a greater power than science, that which makes science, the independency of the individual mind, reigns divinely supreme.

The world at large should recall, as a guide to itself, not less than as an encouragement to scientific healers, what a vast amount of real knowledge is acquired by the physician. To master the details of structure, the anatomy of one organ of the animal body, is as great an effort as to master every detail of many occupations by which, exclusively, some men gain their bread. But the medical man, by infinite labour in a school of the most painful teaching, learns every organ of the body, and knows the body as other men know their houses or factories or machines. The number of diseases affecting humanity are over two hundred, yet are they so well understood by the medical world, that any new form of disease is detected so soon as it makes itself known. The distribution of disease over the planet has been mapped out with such care that the charts of disease are as correct as are those of land and sea which the geographer supplies. So soon as the chemist makes a new discovery of element, or compound, the physician is after it, proves it, and takes it for his use, or rejects it, according to its worth. On these matters of knowledge the true physician bases his practical skill; sick himself, he has no other basis on which to rest. Can the world, except under force of folly and blindness, expect a better or a sounder foundation?

Once more: the world at large should never forget that the progress towards perfection, in medical science, must of necessity be extremely slow, because of the great anxieties and risks attendant upon its cultivation and development. The chemist, who is dealing with inert matter proceeds in his course without any feeling of responsibility in regard to the material substances in his hands; he can play with force and matter alike, indifferently, if he but take care of himself. The mechanist and engineer are in the same fortunate position. But the representative of medicine is circumstanced altogether differently: the wonderful organism which he treats is his own, or the same as his own; towards it he feels sympathy, fear, affection, which are returned to him. Thus, in making every advance, the labour of the physician is infinite, the responsibility of the highest order. He has, probably, to begin by testing his new thought on inferior animals; then perchance, in many cases, he must proceed to himself, and on his own body make his preliminary investigations; and, when all this is achieved, he has to accept the

risk of pronouncing, and bringing into practice, that which he thinks may be done. In this stage of his proceeding his anxieties are greater than ever, for the world at large is wont to look upon him as a theorist, and to make general request that, however valuable his advance may be, it be not tried on them. Neither can he blame the world on this account, it is so natural, though it may stand directly in his way. It is impossible, indeed, to describe how deep is the anxiety with which the thoughtful man moves step, by step, in the progress of new enquiry in medicine. First the mind has to give up something which it has been taught to believe and practise; next, it has to see solid grounds for disbelief, and, by a process of negation, to begin its course. Then it has to weigh every reason why it should suggest new thought in place of the old, and finally, it has to put its new reasoning into the crucible of experience, and to allow learned and unlearned, alike, to be judges of its value. These labours and anxieties are such, the wonder is that progress is made at the rate at which it actually travels. The majority of practitioners, burdened with the weight of forty-seven cases of disease continually on hand, have little leisure for new research, while those who have the leisure, as a general rule, shrink from investigation that lies out of the line of their direct daily pursuits. I put it, therefore, to the members of the world at large to think generously on the advance of medicine, and to be satisfied that if the advance be slow, it is not too slow to be sure and to deserve that confidence which they ultimately will have to repose in it as the partakers of the good it shall achieve. Moreover, and without a syllable against the conservatism men of the world exhibit when they express that they prefer to be in the hands of what they call "the safe man," who tries nothing new, I crave of them not to let the feeling pass into superstition, and injustice.

The world at large should recognise, better than it does, the great fact that, in true scientific medicine, there is no secrecy of knowledge, and should be more cautious than it is in believing that this or that man has some special gift, or information, which less favoured brethren have not. It is quite true there are men endowed with special gifts, resulting from practice or knowledge: one man has a delicate hand and strong heart for the performance of an operation, another man has not, and so far the particular man is to be trusted and rightly preferred in his particular way. But, carried to an extreme, the fashion of the public to specialise all medical men, is painfully absurd, and is opposed to the best interests

of the whole human family. To assume that one physician is, what is vulgarly called, "clever" (a term of positive insult to a man of true science) for this malady, a second for that malady, and so on, is injurious alike to the profession and to the public. It fills the professional man with false pride, leading him from the contemplation of the body as a whole, and from disease as a unity presenting a variety of phenomena, to the contemplation of phenomena individually, and apart from unity. It impedes discovery, which must always be built on unity, because it divides thought and nourishes dogma. It ruins the public judgment, because it tempts the most insignificant mortal into the belief that, if he can only successfully inoculate the public mind with the assumption that he is specially wise on the subject of a given disorder, he can win on such bare and base assumption. And lastly, it prevents men of broad and expanded views from cultivating their particular talents, and enshrouds them with the fear that if they give scope and character to their life, they will be considered, however accurate their learning, however sound their judgment, as too general in their aspirations, and too indefinite in their principles.

In relation to this same subject, the world at large requires to be corrected in regard to the object it has before it, when it seeks the services of the physician or surgeon. Ninety-nine persons out of a hundred, who go to consult a medical man, never think of asking for knowledge, but are content simply to ask for what they call "*an opinion;*" nay, when they change from one medical man to another, they take the step again, almost invariably, not with the desire of knowing more, but of getting "*an opinion*" more. This is fatal error, leading to inconstancy, caprice, and deficient earnestness of purpose. An "*opinion*" can be given by anyone; knowledge can only be supplied by the learned. I am so poor an astronomer I doubt if I could calculate the time of an eclipse, simple as the problem is to the learned in astronomy; but, if it would soothe the feelings of anyone present to have an opinion whether the inhabitants of the planets live exclusively on Welsh rarebits, I would undertake to give an opinion on that subject, particularly if the usual fee were remitted, as sound as could be given by Adams, Leverrier, or Herschel himself. I do not offer these remarks as against opinion, based on knowledge, but only as showing the folly, so widely prevailing, of seeking opinion irrespective of knowledge. True it is that the profession itself, to a large extent, corrects this folly, because medical men, when they meet together, go direct enough to the question, not of opinion but

of knowledge, and test and try each other with a severity which those outside the pale little comprehend. It would be better for the members of the public to imitate this example; and, in hours of trial, before they throw the handkerchief from this physician to that, to return disappointed, as they constantly return, to the physician who knows them best, to consider on what grounds they make the change, why one man should know more than another, and what solid reason gossiping advisers have for upsetting the relationships that ought to exist between the sick man and his conscientious adviser. The present time is one in which these words require to be very decisively spoken. The break in the relationships I have named, is based, often, on such frivolous arguments that the faithful physician is left without heart to pursue rigorously and thoughtfully the course he sees most advantageous to his patient; and, the result is, that he loses interest in his charge, pleasure in his duty. In doubt or difficulty he has not, and cannot have, the slightest hesitation in expressing doubt or difficulty, and in saying to his patient, "I, who know most correctly the capacities of my profession, the true workers in it, should like, if I were in your place, to gather the knowledge which some one of my brethren may possess." But to be allowed no opportunity for such suggestion, to be discarded to-day, and courted to-morrow, equally against reason, is a process morally bad; the good man palls under it, grows cynical, cold, doubtful. The shallow man alone luxuriates in it; the people, says he, want an opinion, and care little for knowledge; why then should I burden myself with knowledge, when opinion will pass for knowledge and wisdom combined?

An intelligent interest on the part of the world at large, an interest which every honest physician longs to see evinced, would lead to a direct reform in the world of physic itself; it would make the profession more secure, the sick safer, the healthy happier. It would inspire that just pride in the members of the profession which is the very soul of true attainment, and would let every man feel he is esteemed for what he is, and not for what he seems to be.

It may be expected, from what I have already said that, I shall forthwith proceed to call in question the consort of the people with quacks; or pass an anathema on quacks and quackery individually and collectively. From this I abstain. Consort of the community with quacks is so obviously the result of ignorance, that, if the most moderate share of attention were given to the subject, if a tithe of the attention I have prayed on behalf of the profession itself were

given to the subject, communion with quacks and their foolish arts would naturally cease. As to the quacks, to notice them were to elevate them. Belonging strictly to the worst of the criminal classes, they are moved by no sentiments which the most acute criticism could touch. A professed gambler may have sense of honour, a professed pickpocket may have skill, a professed burglar may have courage: the professed quack has the sins of them all, the saving qualities of none. He is, because he is permitted, a forced necessity of morbid minds. One thing only would I note in his history as most wonderful: viz; that the grand disseminators of human knowledge, the grand teachers of moral truths, the proprietors of the fourth estate, allow him unblushingly to deface their fair pages with his falsehood, his snares, his open loathsome sin. Day by day the press, in daring faultless language and sentiment, exposes vice and purifies the thought of the world; day by day, in the greater number of its organs, it sells itself to the advertisement of immoralities worse than the worst it endeavours to remove.

THE WORLD OF PHYSIC TO THE WORLD AT LARGE.

In asking the world at large to cultivate a more correct understanding of the world of physic, it were false to assume that the world of physic has no new obligations to perform in return, no new understandings to acquire. Quite the contrary, if it has something it ought to take, it has much also it ought to give, and that without reserve or waiting for primary acknowledgments.

In the first place, the world of physic has arrived at a point when, without dogmatical violence, it should be ready to sustain a firm defence of all its practical part. The time has been when, in medicine universally, there was the same science as that which our still extant Lama physician, with his iron stick, leathern bag, teakettle, garner of vegetables, paper pills, and demoniacal horse, so proudly possesses. In simple enquiry for remedies possessing each a specific virtue, in charms or spells, our ancient fathers of now civilized medicine spent their days. Slowly they gave up the direct superstitions, so slowly that, as late as the reign of the second Charles, there were some who conceived the royal touch a cure for scrofula, and, when that mad monarch went about his bad business into the unknown, there were some, who, with strong dash of superstition hanging to them, must needs cram dead bones, finely powdered,

down the royal gullet into the royal stomach itself. But, in time, the direct superstitions died out, the actual remedies were left to work alone, and physicians of eminence became great in particular remedies for particular diseases. Old Dr. Radcliffe's book of receipts, what a tenacious hold it had! and pharmacopœias sent out from head-quarters, what wonderful forms they supplied! 'Philosophic oil of bricks,' imagine that for the name of a remedy! and broth made of vipers, think of that for a food of curative character!

These things are swept away, and the principle of trying to discover remedies by empirical observation is on the wane. The whole field of medicine is indeed so changed, that, if a physician well informed in all modern advancements could possibly meet even such true men of the last century as Richard Mead, or John Arbuthnott, I doubt if there could be any common understanding between them, either in respect to the nature of symptoms, cause of disease, or question of treatment. Notwithstanding, there is yet great work to be done. Still influenced by the crude hypothesis of the possession of particular remedies for particular diseases, still vain of its method of learning by loose and disconnected observation, the medical mind is weak, watery even, in the knowledge of positive treatment. It flatters itself with what it calls experience, which means individual opinion as to the virtue of some particular thing, tested all on one side, without a single research in the line of disproof, and without the remotest allowance for the almost certainty of coincidence. Or, running into an extreme of disbelief, it flatters itself again on an experience in an opposite direction, experience of doubt as to the positive value of medicinal remedies altogether. Thus it is open to any man to obtain notoriety for any remedy however absurd, and based on hypotheses however foolish, if he will swear hard enough, and loud enough, to the good results which are seen while the patient is under the remedy, and nothing more. Thus it is also open to any man to gain notoriety by, practically, giving no remedies at all, if he will only swear hard enough, and loud enough, to the good results which were observed while the patient was under no remedy, and will be silent on all else.

Let it not be concealed a moment that this state, though it mark only a transitional stage of learning, is all wrong. It makes the profession, where it should be as firm as adamant and as certain as time, loose as sand and uncertain as the wind. Thus circumstanced, the profession gives way to popular cries, and, with blushing face,

bends its head, in mass, even to the ignorant fashions of the hour. For two thousand years one remedy I could name held unrivalled sway. Suddenly, by caprice of experience and fortuity, this remedy, became unpopular; straightway from that side of it which was good its masters turned their ready eyes to that side of it which was evil, and lo! the remedy of two thousand years was let go in a quarter of a generation. In contrast, there springs up another remedy I could name, which for more than two thousand years had held a modest place. Caprice and fortuity now lifted this remedy into the sunshine of favour; straightway from that side of it which was bad its masters turn their eyes to that which was good, and lo! the remedy is so exalted that grinning Bacchus leaves his barrel to find out Esculapius, and congratulate him on the conversion of his family to the right faith at last.

You smile; 'tis a theme on which to be solemn. There can be no true, practical, safe or sound medical skill, no freedom from quackery, no mutual trust between the world of physic and the world at large, until each man of physic can conscientiously say, Whatever I believe, or teach, I know.

You ask me one fundamental question. Of two hundred and fifty well marked series of phenomena known as special diseases, how many, under the proper natural conditions for recovery, would pass into recovery and health without any special remedy at all? I am not ignorant of medicine, but I answer boldly, on that point I have no knowledge. Basic as the question is, corrective of innumerable pretences as the true answer to it would be, it has never yet been systematically studied, nor held worthy of serious regard. The mutual understanding that should subsist between the world of physic and the world on this point, rests for obtainment with the world of physic. It is the interest of medicine, as it is its duty, to save the world the labour of the answer, and to become itself consolidated by the first possession of the truth.

To make, with rapidity and certainty, those advances which shall place the physician in the possession of the positive scientific practical knowledge he requires, the world of physic generally is bound to look a little more keenly at the advances of pure physical science, and at the physical connections which exist between the man and the natural forces which surround him, and, if I may so say, fill him. The thought that man is an isolation, common as it is, is a thought which must be sacrificed to truth. The physician who looks on a living body before him as belonging to itself, to a part of the

earth, to the earth altogether, and nothing more, is no true physician. That body which he touches, moving in its undying course, belongs to the whole universe of God. It breathes, it yields a breath of matter, having substance, weight, even form! What is the destination of that invisible breath? It speaks and sets in motion waves of infinite matter, extending in infinite space, through infinite time: not a breath, not a sound, not a motion, not a thought lost. 'Tis a sublime organism. Yet are we favoured to learn it, as we shall one day know it, a simple organism. An organism now bending under the attraction of mother earth, weak, exhausted; now charged with force, strong, and resisting the attraction. Again, weaker and weaker, and at last so weak that it returns to the grasping mother, senseless as herself, her own. What alterations of force to matter subtend in these phenomena? That is the first question for the modern physician. Drugs, instruments, good drugs, good instruments, bad drugs, bad instruments, they are the same, until they take their secondary place; then, in their place, they are all good. In this, the pure physical line of advancement in medicine, there is promise untold. To follow it, it is not necessary that many men be taken from the toiling ranks: the bases lie soundly for it, and the revolution of thought and practice it will bring, must needs be slow, silent, and attuned to the time.

I do not stop to name minor advances of the world of physic, though many tempt me by the way, but pass, finally, to consider one which refers rather to a neglected than an unrecognized subject; I mean the careful study of moral and mental curative measures as supplementary and conclusive to those which are purely physical. The most scientific physicians have, I think, fallen into the error of studying, with too exclusive a care, the observable conditions of the body healthy or diseased, and those agents or agencies for curing diseases which produce the most obvious effects; such as knives and other instruments, anæsthetic vapours, active drugs, heat and cold, electrical shocks, and the like. I take it, truly, that these are naturally, first, studies. It is fair to infer, from the general plan of creation, that the simplest organism is the primitive, and that the pure physical existence is the ground-work and the primary necessity of the highest form of living thinking thing. But granting this, we, men and women, highest forms on this earth at all events, are, as Plato magnificently teaches, "plants not of earth but of heaven, and from the same source whence the soul first arose, a divine nature, raising aloft our head and root, directs our

whole corporeal frame." While, therefore, it is by nature the first duty of the healer to make that corporeal frame pure and whole, is it not equally his duty to study what shall enter by the senses or windows of the mind, and, though invisibly entering, be potent forces for evil or for good? Because an agency is not visible, not tangible, is it less real? If a man lose his mind by the failure of his blood, that, it is said, is plain to understand, for it is physical. But, if some horror come upon the man through his mind, so that like poor Horatio, he is bechilled

> "Almost to jelly by the act of fear,
> "Stands dumb and speaks not;"

is not that, too, physical; an action direct of mind on matter, reversing the physics of the body and creating disease? It must be so, and in the study of this action from the universe into the man there lies a world unknown. With strange acuteness, charlatans of all kinds have touched, without understanding, this unknown world. They have played, it is said, on the credulity of man; they have done more, they have, in ignorance of what they were doing, touched the animal motion through the direct entrances by which the universal spirit enters also. The need for new contemplation here, increases with the intellectual development of the race. The cave-dweller is gone; the Norseman is gone; the crusader is gone; the animal body, living with powers superior to the soul within it, must also, in the course of the suns, altogether pass away, to be replaced by an organism more finely moulded, more accessible to the external beauty and harmony, more sensitive of pain, more sensible of weakness, less susceptible of maladies evidenced through matter, more susceptible of maladies evidenced through mind, and more impressionable to cure or to injury, through the mind, than through the baser body. To study these changes of existence and action, to open this unknown world of natural truth, not to trade upon the knowledge of its existence, but to comprehend it with the grasp of a philosopher, is a duty to which the man of physic must devote himself with zeal, or recede with humiliation from one of the strongest seats in philosophy.

Our million of physic—scattered over every part of the planet where there is enfeebled humanity to be tended, cherished, and upheld, by night and by day—it, and its relations to the world, its good, its bad, its hopes, its despairs, we can on this

occasion follow no longer. I have tried to present it faithfully. I have tried to show its strength; I have not tried to conceal its weakness. Called forth by the world itself from dire distress, a natural birth, its mere existence proves it to be a useful million of the hundred thousand millions in which it lives and moves. Who follows its work with his whole heart, and for itself, must needs be one whom the best may envy and approve; who follows its work with no heart, and for himself alone, must needs be one the worst may pity and contemn. Its plainest, simplest work, well done, the world of physic must remain in fair comparison with all the world beside; its noblest work realized, it must make all science subservient to its grand intents, and, ultimately, reveal the mystery of life with a crystal clearness and simplicity that shall render only the more wonderful, and the more sublimely inapproachable, the wisdom of the "uncreated, super-essential, and all-beauteous mind."

ON THE CRIMINAL RESPONSIBILITY OF THE INSANE.

By T. Harrington Tuke, M.D.

Mr. President and Gentlemen,—It was with very great pleasure that I supported the resolution of your Council, to choose for discussion at our Annual Meeting, the vexed question of the responsibility of the criminal insane to punishment

Honoured as I feel by having been requested myself to bring this question before you, I cannot but confess that my own decided leaning to one side in this controversy, leads me to doubt whether your Council have made the best selection amongst our members, in inviting me to open this debate. The subject, however, is one in which I have been much practically engaged and interested. I will do my best to bring it fairly before you, being more anxious to elicit your views than to press my own; and if the opinion of so influential a body of practical medical men, as those I am now addressing, should be in unison with mine, it will much strengthen the hands of those who think as I do, that the present state of the law in relation to Criminal Lunatics is a blot upon our legislation, leading, in many instances, to great injustice in the infliction of punishment, more especially in the cases of lunatics convicted of murder.

The decision of this Association will have more value when it is remembered that, as I believe, this question is for the first time submitted for discussion to a body of practical physicians, not specially connected with the treatment of the insane. I am afraid it must be admitted, however distasteful such an admission will be to some of my brother psychologists, that our efforts, as specialists, to amend the law, have been singularly unsuccessful. The theoretical views, the metaphysical subtleties, the dogmatic opinions expressed in the witness-box by some of our body, have caused the name of "mad doctors" to be viewed with suspicion by jurymen and judges. And yet, I think, I can prove that the enactments of the law, as expounded by lawyers of modern days, in one essential point at least,

are opposed not only to justice, but to reason and common sense. I propose to bring this prominently before you, to confine myself, as much as possible, solely to its examination, and to the proof that this conflict between law and common sense, arises mainly from the entire ignorance of lawyers as to the nature of insanity considered as *disease*. It may seem bold to challenge the decision of twelve judges, but it must be remembered that their opinion was not asked as to the *justice* of the law in condemning a certain class of criminals to death, but as to the tenor of certain statutes in relation to their offences; that even then they were not agreed; and that before and afterwards, some of them, in charging juries, opposed their own former reading of the law.*

It has unfortunately happened that public discussion and interest on the question of the responsibility of the insane to punishment, have been generally called forth only on the particular occasions of some specially terrible crime or great scandal. Men have talked much of the Windham or the Townley cases, have speculated upon verdicts, or discussed individual judgments, while the abstract principle has remained neglected. In the following remarks, allusion to special cases will be avoided as far as possible, and two distinct issues alone argued. First, that all intellectually insane men should be exempt from capital punishment for crime. Secondly, that the present state of the law on this point is contradictory and opposed to the teaching of medical science.

An attempt is being made by my learned friend, Professor Laycock, with more prospect of success than such attempts have hitherto had, to induce the Government to issue a Royal Commission of Inquiry into these and kindred questions connected with mental disease. Dr. Laycock indicates as most worthy of inquiry for such a Commission, the alleged increase of lunacy, the relationship between lunacy and crime, the prevention, as far as possible, of insanity by hereditary transmission. These questions are most important, although foreign to my present purpose; but Dr. Laycock proposes also for special inquiry by such a Commission, the state of the law regarding the responsibility of the insane. In obtaining this, I think, any decision, of our Association in unison with his views would materially assist him. Professor Laycock's views are ably and lucidly set forth in a paper read by him at the Medico-Psychological Association, and published in its journal and in the *Lancet*.

* Chief Justice Tindal in McNaughten's case, and Justice Williams in that of Frost.

Speaking to practical men, and aiming only at a definite result, or, at least, at clear expressions of assent or dissent to my general propositions, I will, as much as possible, avoid any technical discussion, dispute as to terms and definitions, or digression into collateral questions. I therefore propose, *per viam exclusionis*, to bring before you exactly the point upon which I ask your opinion by first specifying those which I wish to avoid. I beseech your indulgence for this course. I am anxious to arrive at a practical conclusion, and I see legal and other friends who would willingly splinter a lance with me upon certain questions, which would inevitably lead to a *melée*, in which the main object of our present contest, if contest we should have, may be lost sight of and forgotten.

And first, I would ask this meeting entirely to ignore the question as to the existence or non-existence of a disease called "moral insanity." I have frequently seen the symptoms of deranged moral feeling with those of intellectual disturbance; I have been consulted in cases of children who seemed in undue measure wicked or unruly; but I have always imagined that in these cases the *physique*, and not the *morale*, was at fault, and I have known these children grow up morally sane, as they became older, because then intellectually stronger. Moral insanity without intellectual disturbance *may* exist; but in the experience of my life, and I have since childhood been in constant familiarity with the insane, I have never seen it, and in the only case in which I ever heard it alleged in extenuation of crime, the "expert" considerably weakened his evidence by the declaration that he found delusion also; and his testimony having been conclusively controverted on this point, it is probable he was as wrong upon the other.

For another, though very different reason, I would exclude from our present examination, homicidal, sudden, or impulsive mania; not that, as in the former case, I do not admit its existence without intellectual disturbance, but because its discussion might lead up to another issue, and to a wider field of inquiry. I may say, however, that it must be in the knowledge of us all, that acts of impulsive criminality or folly do arise from certain morbid physical conditions, especially in the female organization; and although the plea of impulsive and irresistible crime, like any other, may be grossly misused, it must some day or other be recognised as a fact, even by those stern dispensers of the law who ask blood for blood, who speak by the card, and hear with aversion the

teaching of science, or the lessons of practical medical experience, if they clash at all with the dogmatic utterances of the bar.

Excluding moral insanity, and homicidal or suicidal impulse, excluding, in fact, emotional and volitional aberration of intellect, it follows that there is only intellectual insanity, or undoubted derangement of mind remaining; and it might be imagined that there could be no difficulty in procuring the escape of a prisoner from the infliction of capital punishment at least, by the production of evidence of his being in a state of mental derangement.

Upon this subject there is a wide misapprehension in the public mind. They imagine the question in all disputed cases to be simply that of insanity or sanity. The popular mind, with intuitive, yet perfect justice, cannot conceive the execution of a lunatic to be possible. "Do you think," asked Lord Stanley of a medical witness before the Royal Commission on Capital Punishment, "that there is any possibility of a man being tried, convicted, sentenced, and executed upon a capital charge, that man preserving the appearance of sanity, and yet being insane?" The answer was direct, "I have myself seen such a case." In answer to further questions, the witness stated that strong representations as to the insanity of the convict in question were made to the Home Office. Yet although the existence of delusion in the convict's mind was not denied, he was executed on the ground that the evidence led to a presumption that he knew the difference between right and wrong. This statement of the witness was not challenged by the then Home Secretary, an active member of the Commission, who was present; nor, indeed, could it be disputed. I venture to repeat that in this case, to my own knowledge, an insane man was hung; and I say moreover, that, perhaps, in the existing state of the law, certainly by the present ruling of the judges, or of many of them, the plea of insanity is set at nought, and an irrelevant issue is proposed to the jury when they are asked not to decide as to the insanity of the criminal, but as to his knowledge of right and wrong.

It must be understood that in limiting the inquiry as to the responsibility of the insane to cases of intellectual insanity only, I have done so for the purpose of making the legal error more plainly apparent. It is obvious that an insane criminal may have disorder of the will, of the affections, or of the intellect, or derangement of all these simultaneously. But now suppose the case of a man arraigned for murder, who is intellectually insane, with or without delusions, or in whom there are such physical

symptoms as to lead a medical man to the conviction that he has disease of the brain.

Another important popular misapprehension is of great importance. It is often imagined that experts in lunacy advocate the entire irresponsibility of lunatics. This is not the case; at least, I do not, nor would I ask you to entertain such a proposition. Society must be protected, but that may be efficiently done without hanging lunatic homicides. Except in those rare cases in which, as in puerperal mania, murder is the result of distinctly temporary disease of the nerve centres, insane murderers should be confined for life, although regarded not as criminals to be punished, but as sufferers to be pitied, and if possible cured, though never again to be released.

The English law upon the subject of executing the insane is thus laid down in Blackstone's Commentaries; the passage is short, and singularly clear and precise in its phraseology. In expressing any dissent from the present legal doctrines, this passage from Blackstone will at least prove that I am no innovator, since my object is not to change the law, but to revert to the ancient reading of it by its former accredited exponents.

"The second case of a deficiency in will which excuses from the guilt of crimes, arises also from a defective or vitiated understanding, viz., in an *idiot* or a *lunatic*. For the rule of law as to the latter, which may easily be adapted also to the former, is that 'furiosus furore solum punitur.' In criminal cases, therefore, idiots and lunatics are not chargeable for their own acts, if committed when under these incapacities; no, not even for treason itself. Also, if a man in his sound memory commits a capital offence, and, before arraignment for it, he becomes mad, he ought not to be arraigned for it, because he is not able to plead to it with that advice and caution that he ought. And if, after he has pleaded, the prisoner becomes mad, he shall not be tried: for how can he make his defence? if, after he be tried and found guilty, he loses his senses before judgment, judgment shall not be pronounced; and if, after judgment, he becomes of nonsane memory, execution shall be stayed: for, peradventure, says the humanity of the English law, had the prisoner been of sound memory, he might have alleged something in stay of judgment or execution."

If this exposition of the English law were the practical guide in our courts of jurisprudence, in my opinion the difficulties that now

arise would be much lessened, or even disappear; the question would be reduced in every case to the proof or disproof of insanity, and the English law procedures would become consistent with medical science. The first question should be, "What constitutes insanity?" The second, "Is such insanity present in the case under examination?" In modern days a third issue has been raised, which appears to me inconsistent with the spirit of the law, however consonant with the letter, and although certainly practically inoperative in many cases, is nevertheless warmly advocated by the legal profession, and has almost entirely the support of the general press. The exposition of the law as I have read it continued, although in a few cases a more severe reading was enforced, up to the trial of McNaughten, who was arraigned at the Old Bailey, as many of us remember, in the year 1843, for the murder of Mr. Drummond. This terrible crime, involving the sudden death of a well-known and esteemed public servant, led to much disagreement and discussion. We know now, by his after conduct, that McNaughten was unquestionably of unsound mind. Nevertheless his acquittal was the occasion of much angry comment, and the conduct of the Chief Justice Tindal, who virtually stopped the case on the ground of the insanity of the prisoner, was severely criticised. Nine medical witnesses, the last two of them strangers to the case, and only accidentally present in the court, were examined, and gave positive testimony as to the insanity of the prisoner at the bar. Upon this the Lord Chief Justice inquired of Sir William Follett, the Crown Prosecutor, whether he was prepared with contrary evidence; "because if you are not," said the Chief Justice, who was sitting with the Justices Williams and Coleridge, "*we think we are under the necessity of stopping the case.*" This decision, so entirely in accordance with Blackstone's views, and one that the after history of Macnaughten showed to have been so equitable, occasioned great disquietude, and the legal profession were especially much outraged. It led to the following questions being submitted to the twelve judges, which with their answers are here appended.

QUESTIONS PUT TO THE JUDGES BY THE HOUSE OF LORDS, IN RELATION TO THE LAW RESPECTING ALLEGED CRIMES COMMITTED BY PERSONS AFFLICTED WITH INSANE DELUSION.

"1. What is the law respecting alleged crimes committed by persons afflicted with insane delusion in respect of one or more particular subjects or persons: as, for instance, where, at the time of the commission of the alleged crime, the accused knew he was acting contrary to law, but did the act complained of with a view, under the influence of insane delusion, of redressing or revenging some supposed grievance or injury, or of producing some public benefit?

"2. What are the proper questions to be submitted to the jury when a person, alleged to be afflicted with insane delusion respecting one or more particular subjects or persons, is charged with the commission of a crime (murder, for example), and insanity is set up as a defence?

"3. In what terms ought the question to be left to the jury as to the prisoner's state of mind at the time when the act was committed?

"4. If a person under an insane delusion as to the existing facts commits an offence in consequence thereof, is he thereby excused?

"5. Can a medical man, conversant with the disease of insanity, who never saw the prisoner previously to the trial, but who was present during the whole trial and the examination of all the witnesses, be asked his opinion as to the state of the prisoner's mind at the time of the commission of the alleged crime, or his opinion whether the prisoner was conscious at the time of doing the act, that he was acting contrary to law, or whether he was labouring under and what delusion at the time?"

ANSWERS OF THE JUDGES.

"1. Assuming that your Lordships' inquiries are confined to those persons who labour under such partial delusions only, and are not in other respects insane, we are of opinion that notwithstanding the party did the act complained of with a view, under the influence of insane delusion, of redressing or avenging some supposed grievance or injury, or of producing some public benefit, he is nevertheless

punishable, according to the nature of the crime committed, if he knew, at the time of committing such crime, that he was acting contrary to law, by which expression we understand your Lordships to mean the law of the land?

"2 & 3. That the jury ought to be told in all cases that every man is presumed to be sane, and to possess a sufficient degree of reason to be responsible for his crimes, until the contrary be proved to their satisfaction; and that, to establish a defence on the ground of insanity, it must be clearly proved that at the time of the committing of the act the party accused was labouring under such a defect of reason, from disease of the mind, as not to know the nature and quality of the act he was doing, or, if he did know it, that he did not know he was doing what was wrong. The mode of putting the latter part of the question to the jury on these occasions has generally been whether the accused, at the time of doing the act, knew the difference between right and wrong, which mode, though rarely, if ever, leading to any mistake with the jury, is not, as we conceive, so accurate when put generally, and in the abstract, as when put as to the party's knowledge of right and wrong in respect to the very act with which he is charged. If the question were to be put as to the knowledge of the accused, solely and exclusively with reference to the law of the land, it might tend to confound the jury, by inducing them to believe that an actual knowledge of the law of the land was essential in order to lead to a conviction, whereas the law is administered upon the principle that every one must be taken conclusively to know it without proof that he does know it. If the accused was conscious that the act was one which he ought not to do, and if that act was at the same time contrary to the law of the land, he is punishable, and the usual course, therefore, has been to leave the question to the jury, whether the party accused had a sufficient degree of reason to know that he was doing an act that was wrong; and this course, we think, is correct, accompanied with such observations and explanations as the circumstance of each particular case may require.

"4. The answer to this question must of course depend on the nature of the delusion; but making the same assumption as we did before, that he labours under such partial delusion only, and is not in other respects insane, we think he must be considered in the same situation as to responsibility as if the facts with respect to which the delusion exists were real. For example, if under the influence of his delusion he supposes another man to be in the

act of attempting to take away his life, and he kills that man, as he supposes, in self-defence, he would be exempt from punishment. If his delusion was that the deceased had inflicted a serious injury to his character and fortune, and he killed him in revenge for such supposed injury, he would be liable to punishment.

"5. We think the medical man, under the circumstances supposed, cannot in strictness be asked his opinion in the terms above stated, because each of those questions involves the determination of the truth of the facts deposed to, which it is for the jury to decide; and the questions are not mere questions upon a matter of science, in which case such evidence is admissible. But where the facts are admitted or not disputed, and the question becomes substantially one of science only, it may be convenient to allow the question to be put in that general form, though the same cannot be insisted on as a matter of right."—*Archbold's Pleading and Evidence in Criminal Cases*, 15*th ed.*, 15-17.

Thus in the opinion of the twelve judges, Justice Maule being dissentient, the true test of responsibility was not the presence of insanity, but the presence or absence of a supposed "sense," moral or intellectual, which would enable the patient to discriminate between right and wrong. The difficulty that must arise in discriminating between "conscience" and "moral sense" and "knowledge of right and wrong," and demonstrating the soundness of either, even in a sane mind, and *à fortiori* in the mind of a lunatic, is temperately and conclusively pointed out in Dr. Bucknill's admirable essay on the "Responsibility of the Insane," which obtained the Sugden Prize in 1856—a work to which I am myself much indebted, and which every writer or thinker on this subject should consult.

It was in vain that Justice Maule was dissentient; it was in vain that the Chief Justice, who formally reported these opinions to the House, as the result of the study of the *lex scripta* of England by the twelve judges, had very recently saved a lunatic murderer from condemnation, who *did* know right from wrong, or, at least, did so to all human appearance; the lawyers had now a "ruling," a "precedent," a tangible guide which has been referred to, and acted upon, more or less ever since. I will not weary you with instances in proof: a very striking one came under my immediate observation as a witness for the Crown. In a trial for murder before

one of the most able of our judges, the counsel for the prisoner opened before the jury a defence upon the ground of the prisoner's suffering under delusion, that is, intellectual insanity, at the time of his trial. Before any witnesses were called the learned judge interposed, and thus addressed the counsel:—"I have not sat here to try this case without well considering the question I should leave to the jury, and I will now tell you what I have written down, and propose to leave to them. I shall tell them that if they believe that although there might have been *disease of the mind* to some extent, yet if the prisoner knew that the act he was committing would probably cause death, and the doing it would subject him to legal punishment, there was criminal responsibility."

I could give from personal knowledge other instances, but they must be familiar to you. I would ask, is it not contrary to the feeling, to the instinct of the physician, that a man with "disease of mind," and these are the words of the judge, should be condemned as a felon, in spite of our common reason and humanity, and in defiance of the healthier dicta of the law, as given by earlier jurists?

The test of responsibility gauged by the knowledge of the prisoner of the difference between right and wrong, is an absurdity. Madmen, as a rule, if they are not demented or idiotic, or raving, perfectly understand the distinction between the lawful and the unlawful; that this is so, must be in the knowledge of most of us. Moreover the experience of those specially engaged in the care and treatment of the insane, is to the same effect, as the resolution adopted at a recent meeting of the Medico-Psychological Association amply testifies. The resolution is in these words, "That so much of the legal test of the mental condition of an alleged lunatic criminal, as renders him a responsible agent, because he knows the difference between right and wrong, is inconsistent with the fact well known to every member of this meeting, that the power of distinguishing between right and wrong exists frequently in those who are undoubtedly insane, and is often associated with dangerous and uncontrollable delusions."

Now this resolution I would ask you to indorse with your approval. It is a simple statement of fact; but being so, it is antagonistic to the ruling of the twelve judges, although in agreement with the law as laid down by Blackstone and adopted by many of our ablest judges. At a large meeting of medical men engaged in the treatment of the insane it was passed unanimously;

I trust it may also receive the approval of those Graduates of our old University who are now present.

I must not be thought to assert that English judges are less merciful than English physicians, or that extreme legal views always involve the hanging of lunatic murderers. On the contrary, the judges, in many instances override their own view of the law. * Thus, Mr. Justice Williams, in the case of Frost, said the question here is not so much whether the "prisoner knows right from wrong, but whether he was deranged or not at the time he committed the offence." Mr. Justice Montague Smith, in a recent case, intimated to the jury that in his opinion the lunatic at the bar was not competent to plead; and in the case I have already referred to, in which the judge laid down the law before witnesses were called for the defence, he afterwards saved the prisoner's life by expressing to the Home Secretary a wish for further inquiry—a wish arising, as I thought at the time, from his belief as to the prisoner's having "delusions," although his ruling to the jury was that the presence of such delusions was immaterial to their verdict.

It need hardly be pointed out, that this mode of dealing with insane criminals is entirely at variance with the spirit of English law, the question as to the insanity of the prisoner should be decided by a jury; the sentence of capital punishment must not depend upon the humanity of individual judges, or the flexibility or otherwise of the Home Secretary.

Sooner or later the true solution of the difficulty attending this question will be arrived at, when the English system of engaging experts approaches more nearly to that of France. There, in cases of murder by an alleged lunatic, physicians are sent by Government to examine and report as to his mental state, and their opinion, and the reasoning upon which it is founded, is submitted to the jury. The question as to whether a knowledge of right or wrong exists is not raised, it is known to be immaterial; the existence or non-existence of disease is the sole question; and this, in a great degree, is properly left to the technical knowledge of the physicians, who appear in their proper place as advisers of the Court, and not as advocates for or against the prisoner.

I could say much upon the defects of our system of engaging medical witnesses, or "experts," who may become, to some extent, partisans, unconsciously perhaps, but not the less certainly; the question is foreign to my present purpose, which is simply to ask for your opinion as to the propriety of that dogma of the law, or

that exposition of it by the twelve judges in the year 1843, which makes it possible to hang a lunatic, unless medical witnesses can be found to swear that the prisoner under trial did not, at the time of his committing murder, know the difference between right and wrong, or that he has subsequently lost such knowledge, and is therefore irresponsible.

The province of the medical witness is to discover the existence of mental disease if it exists, or to demonstrate its absence if it does not. Disease may be marked by physical symptoms, or, if you will, by moral disturbance, or by emotional or volitional aberration; but the true test cannot be the knowledge, so common to all "reasoning" lunatics, of the difference between right and wrong. An opinion expressed by this numerous and influential body of medical men as to the absurdity of the legal view of the case may be most useful, a universal expression of opinion from the medical profession at large would be conclusive; the lawyers must be wrong, and either the English law should be altered, or the exposition of it by the twelve judges should be declared erroneous.

DISCUSSION ON DR. TUKE'S PAPER.

Dr. Davey.—When I recall my personal experience of the subject to which Dr. Tuke has invited our attention, my first impression is one not unmixed with surprise; surprise that untruth and error should still hold the ground that they do, in so far as the question of the criminal responsibility of the insane is concerned. Mention is made in the paper just read to us of the trial of Daniel McNaughten for the murder of Mr. Drummond in 1842. I took a deep interest in this case, and was present at the Old Bailey during the two or three days it lasted. Those of you who count the number of years I do, will remember well the extreme, even morbid, interest with which this trial was discussed, and will

have no difficulty in calling to mind the burst of indignation which accompanied the poor lunatic's award of "Not Guilty, *on the ground of insanity*." The press, with hardly an exception, reeked with vengeance, and pronounced the issue absurd, dangerous to society—and so on. In a pamphlet written by myself at the time, entitled "Medico-Legal Reflections on the Trial of Daniel McNaughten," I did my best to counteract the pernicious, because erroneous, judgment of the newspaper press; whilst I strove to demonstrate the physiological basis on which enquiries into the mental state of persons charged with criminal acts did really rest. McNaughten was defended by Mr. Cockburn, and the poor fellow owed, no doubt, his rescue from an ignominious end to the able advocacy of that gentleman. He, Mr. Cockburn, broke new ground on the occasion referred to, and his sound and logical positions, and not the less his happy illustrations of the dependency of many of the acts and much of the conduct of the insane, not on the failing or deficient understanding, nor on the incapacity to distinguish right from wrong, but rather on the depraved and disordered moral feelings—that is to say the ever active emotions or affections and the propensities—cannot have been altogether lost to the cause of truth and humanity, even at this day. "Moral insanity," so called, is a fact so palpable and so easily proved to exist within the wards of any lunatic asylum, that the wonder is it could for so long a time as it did escape the attention of observers. The most simple and superficial analysis of not only the morbid mind, but of society itself, must assure us of the profound truth of the position here taken. The character of each of us present is, in the main, dependent on, or caused by, the force or measure of the moral qualities of our being, their conjunction or mutual operation, under any given and objective conditions. The motives to action in every man, his promptings to good or to bad actions, to virtue as well as to vice, spring out of his emotions or affections; and although these are influenced or modified by the perceptive and reflective faculties to an extent commensurate with their inherent and original strength and development, nevertheless does the fact, as I have stated it, remain untouched. The principle is the same among both the sane and the insane; its recognition is, ere long, certain. The legal profession and the public must, in this case, bend to medical science; the mere word of man to the *law* of his Creator. The agitation begotten by McNaughten's case led, as Dr. Tuke has said, to the

exchange of certain questions and answers between the peers and judges. These questions and the replies thereto were made public on an early day in July, 1843; and on the 5th (July) I ventured to read a paper "On Criminal Insanity" before the Phrenological Society of London. In this paper I subjected the opinions of the said peers and judges to a strict medical analysis, and with the effect, as I conceive, of proving both their general and scientific worthlessness. My paper is to be found in vol. 1 of the "Zoist," a publication which, a quarter of a century since, commanded no small circulation. From that time to the present I have given an uninterrupted attention to the subject of crime and insanity, and have had printed for private circulation many hundreds of pamphlets and papers bearing on it; whilst, as I believe, I have never lost an opportunity of urging its due consideration on my medical friends and others interested in the onward course of truth, and the acceptance everywhere of a sound humanity. Before I sit down let me call the attention of gentlemen who hear me to a very important item bearing closely on this matter, and in strict relationship to the position of the lawyer and doctor in our law courts. How much longer are we to witness the very objectionable method of making sides to questions of medical and general science? I can fancy nothing so calculated to defeat the ends of justice as the mode now adopted. Just fancy the absurdity of going into the medical market to purchase, for a money consideration, the opinions of Dr. A. and Dr. B., and so on, with a dozen other medical men, in any given case or question involving the innocence or guilt of a fellow creature! Fancy, too, these several good men and true—medical celebrities—arranging themselves, some on one side and some on the other, half of them paid advocates in the cause of the plaintiff, and the remainder in a like position bent on maintaining the supposed interests of the defendant! Is not such a state of things artificial, and out of harmony with what should be accepted as the legitimate position of medical witnesses in our law courts? You will agree with me, I am sure, when I add that a *medical arbitration* should alone decide the many and abtruse questions so commonly arising, and involving as these do, and must necessarily, the most serious and vital conclusions, both in regard to the individual and society. This Association will, I hope, having opened up this important subject of "moral insanity," be prepared to pursue it to a good and wise end.

Dr. Crisp said that he believed a great many persons had been hanged in this country who were not accountable for their actions. Some years since, at the Medical Society of London, he heard Dr. Forbes Winslow mention the following case. A beggar in Essex asked a farmer one day for two pence, which was refused; the next day the beggar waylaid the farmer, and murdered him. Dr. Winslow and the late Dr. Baily were sent by the Government to examine this man, and they reported that he was insane. Lord Palmerston's reply was, "*Mad or not mad, he shall be hanged*," and he was executed accordingly. He, Dr. Crisp, had long thought, and had expressed this opinion ten years since in his "Carmichael Essay," that a Commission of Lunacy consisting of six or twelve persons appointed by Government, should, in cases of doubt, decide on the soundness or unsoundness of mind of the criminal. A man is tried by a jury of his countrymen, and surely he should have the same benefit where there is a doubt about his sanity.

Mr. William Tallack, Secretary of the Howard Association, (late Secretary of the Anti-Capital Punishment Society,) expressed his cordial approval of Dr. Tuke's paper and of the principles embodied in it. He reminded the meeting, that the Royal Commission on Capital Punishment, after inviting the evidence of several witnesses, of whom Dr. Tuke was one, on criminal lunacy in homicidal cases, took no action in the way of any specific recommendation, further than a united suggestion to Parliament to investigate this subject further and on broader grounds.

Mr. Tallack thought it would be advisable to make this suggestion of the Commissioners one of the pleas in urging, during the ensuing session of Parliament, or as early as practicable, on the attention of the Government, the appointment of a Royal Commission on the whole question of criminal lunacy, both in connection with homicidal crimes, and others. He considered that the time was come for definite attempts to obtain legislative action in this direction.

For many years past the medical profession has been abundantly informed on the question of criminal lunacy, by means of valuable papers which have been published in the usual professional journals or in general reviews, as for instance, in the "Social Science Review," so ably edited by Dr. B. W. Richardson; but the attention of the outside public and of the legislature has only been called to the subject in a very slight degree. If, however, the medical profession, by means of its various corporate bodies, were

to institute prompt and active efforts for bringing their parliamentary "interest," not inconsiderable in the new House of Commons, to bear on the obtaining of a Royal Commission, then an important and definite practical step would be taken towards the solution of the present medico-legal difficulties.

Mr. Tallack also alluded to a misunderstanding by the general public as to the recognized views of the medical profession in regard to criminal lunacy, in consequence of a frequent ambiguity in the use of the word "responsibility" as applied to lunatics, and especially in homicidal cases. When the "responsibility" of such was denied, it was often hastily assumed by the public that absolute immunity was pleaded for. This serious and mischievous misapprehension needs to be corrected by frequent explicit statements as to the limits and extent of lunatic "responsibility." Whilst a homicidal lunatic ought not to be held responsible for the consequences of his acts, however terrible, to the extent of *forfeiting his life*, or of undergoing *disgraceful penal* inflictions, yet it should be most clearly admitted, that, in every such case, there should be the utmost responsibility to the public, so far as the forfeiture of *liberty* and the right of imposing *perfectly secure restraint* are involved.

Mr. Tallack suggested that the St. Andrews Medical Graduates' Association should forthwith set an example of effort to obtain a Royal Commission by at once memorializing the Government to that effect, and using its influence to secure the co-operation of its parliamentary friends.

Dr. Billing said that he entirely agreed with Dr. Tuke in the lucid exposition he had given of the subject, which was consistent with the opinions of medical men of experience, who were well aware that insane persons who had betrayed aberration on only one subject, were liable to an extension of the disease to other subjects, and that although a monomania might appear harmless, there was no guarantee that a very dangerous addition might not be made to it, either as to life or property, because disease of the brain, as of other parts, was liable to spread. Besides which, that disease of the brain, not previously evidenced otherwise, might give rise to sudden violent or preposterous actions. All this is well known to competent medical men, but as barristers, and judges even, are ignorant of the physiology and pathology, or the healthy and diseased actions, of the brain, they run into errors in examinations and cross-examinations of witnesses, and in charges to juries, suffi-

cient to pervert common sense and justice as to the responsibility of the insane.

The President thought it would be well to present a memorial to the Home Secretary, setting forth the views of the Association on this important matter.

Mr. Hume Williams regretted his inability to have been present at the early part of the discussion. There could be no doubt that great practical good would follow from the investigation of obscure points of psychology in relation to law, for the appreciation of which those present were so eminently qualified. To use the words of Sir H. Jenner, "It is the presumption of law that every man is of sound mind till the contrary is shown," and did the law abide the result of such showing, there would be no cause of complaint. Unfortunately, it was otherwise. The law too frequently, in its adherence to the rule of the fifteen judges, begged the question of sanity, and assumed the legal test of "a knowledge of right and wrong," as equivalent to a sound condition of mind in relation to the act in reference to which such knowledge was found to exist. This would bring the inmates of many asylums within the operation of the law, and shake to their centre every principle of psychological practice. Its adoption as a legal rule, was an illustration of the apparent antagonism in principle and practice which occasionally produced scepticism in medicine. The exceptions to the general position were so numerous, and generally so startling, that all experienced in the treatment of mental diseases agreed that its application was unsuited to a vast majority of those cases in which the apparent identity of insanity and crime beset the investigation with special difficulty. Psychology has made a great advance since the trial of McNaughten. Emotional or impulsive insanity is now better understood, as well as that form of obscure disease which eventuates in what has been not inaptly termed "motiveless crime." Many recent trials prove that insanity existed to a greater extent than was generally believed. Cases are frequent in which a low moral nature, with strong animal passions, leave individuals subject to influences to which the mere brute was stranger. It was thus that not a few crimes might be explained where no adequate or reasonable motive existed for the perpetration of the most savage and terrible acts of hatred or revenge. In all such cases, the opinion of experienced experts—physicians accustomed to treat diseases of the mind, and familiar with its Protean forms—should be consulted. It was sad to hear

physicians without such experience occasionally giving evidence in cases in which human life was not unfrequently the price paid for their misapprehension of human suffering. On all such trials the resident medical officer of the district asylum should be required to examine and report on the condition of the accused. This was one extreme. Another, and scarce less deplorable extreme, was where property rather than life was at stake. Many recent trials have directed attention to that condition described by Sir J. Nicholl as "imbecility of mind." "Imbecility and weakness of mind," says that learned judge, "may exist in different degrees, between the limits of absolute idiotcy on the one hand, and of perfect capacity on the other; 'weakness of mind,' as distinguished from 'insanity.'" The difference was real. The inability to manage property might co-exist with a capacity for moral observances; and such inability be distinguished from inexperience or unsoundness of mind. It was a condition in which the Romans would have appointed either a "tutor" or "curator." The English law had no such provision. Youths might squander their hereditary estates amongst prostitutes and prizefighters. Middle-aged ladies disinherit their relatives, and leave large properties to designing strangers. Vanity or folly might reduce to poverty the weak and trusting, and the protection of our law be withheld from the prohibition of such organised frauds as recent examples have shown to be in daily operation. At present, the fool is judged according to his folly. Admitting all such cases to involve principles of personal liberty, and not to be without that benefit to the community to which Blackstone alludes as resulting from prodigality; surely psychological physicians, those now assembled, recognised therein a morbid mental condition to which medicine might fairly call the aid of law, and both act for the public, as well as personal advantage of those so circumstanced. Much good could not fail to result from the full discussion of such conditions. Mr. Hume Williams believed that the voice of the medical profession would prove powerful if directed to the prevention of occurrences similar to those which had recently caused so much private and public scandal.

Dr. Tuke.—I am obliged to the members of our Association for their expression of general concordance with the views as to the responsibility of the insane, which I have, imperfectly I fear, brought before you. The suggestion of the President is marked, as all his propositions are, with the promise of practical usefulness,

and I shall be very happy to join with him and with our Council in drawing up a memorial, which, presented in the proper quarter, will I believe receive due attention, as containing the opinion of a number of experienced practical physicians.

I have confined my attention in the paper I have had the honour of reading to you, exclusively to the subject of capital punishment; but in reference to the remarks of my friend, Mr. Hume Williams, whom we have all listened to with much interest, I would add, that the question of the responsibility of the insane to punishment for minor crime is also well deserving of attention; and the law in regard to it is altogether, in my opinion, unjust. The acquittal of a prisoner upon the plea of insanity involves his imprisonment during the pleasure of the Crown, which may be for life. I would instance the working of this law. I gave some short time ago evidence as to the existence of physical brain disease in a prisoner arraigned on a charge of forgery, (he had forged a woman's name to a post office order, and presented it himself). Other witnesses deposed to various acts of insanity. The judge told the jury that as the prisoner had undoubtedly committed the forgery, *the punishment to be inflicted upon him would be less severe* than his imprisonment upon acquittal on the ground of his insanity. I may mention that my medical evidence was commented upon in the way that judges and lawers generally comment upon things they do not understand. The jury, with much discretion, astonished the court by finding the prisoner "not guilty," refusing to add their reason; thus compelling the judge to discharge him absolutely. This unhappy gentleman died within a few months. Under our existing law, he narrowly escaped dying as a felon in a prison infirmary.

I have to thank Mr. Tallack for his remarks. I was very glad to see him here to-day; and I trust that his labours, in the same field to some extent as my own, may long continue.

I have to thank the members generally for their attention and support.

Dr. Davey thought with the President that some practical effect should follow that day's discussion, and therefore moved,—

> "That the Council be instructed to consider whether a Memorial on the question of the criminal responsibility of the insane should be presented to the Houses of Parliament."

Dr. H. Day seconded the motion, which was carried unanimously.

A CASE OF IMPERFORATE ANUS SUCCESSFULLY OPERATED UPON.—WITH REMARKS.

By D. Lloyd Roberts, M.D., M.R.C.P. Lond.; Surgeon to St. Mary's Hospital, Manchester.

William Henry C., born at the full term, on the 11th of June, 1868, at 8 a.m., is a stout, healthy-looking child. His parents are both healthy. He is the eighth child, and all the others are perfectly formed. Whilst washing him the midwife noticed the absence of the anus. In every other respect the infant appeared perfectly formed. It was brought to me about 7 o'clock p.m., of the day of its birth, and on examination of the parts there did not appear to be a vestige of an anal orifice. Moreover, no discoloration or elevation of the skin existed—not even a pucker of the skin to indicate an anal orifice. The raphé of the perineum was more prominent than normal, the abdomen was tense and flatulent, and the child cried and appeared to be in pain. Pressure on its abdomen seemed to cause pain and straining. The child had experienced some nausea and vomiting. It had passed water, and had partaken of some milk and water. The nature of the malformation was explained to the father; who was told that no relief was possible, except by means of an operation; and that, moreover, the chance for the child was very unfavourable. The father consenting, it was decided that the child should undergo the operation. Accordingly, in the presence of my friends, Messrs. Runcorn, Kershaw, and Barnish, I made an incision in the medial line of the perineum, in the position where I thought the rectum would be situated. Dividing the skin and subcutaneous areolar tissue in the direction of the coccyx, I continued the incision for about an inch and a half upwards into the pelvis. An exploratory examination was now made, first with the little finger, and then with a gum-elastic bougie. I thought I detected fluctuation, and

accordingly plunged a large curved trochar and canula into the fluctuating spot. On withdrawing the trochar I was gratified to see meconium, accompanied with a little blood, freely flowing through the canula. This was allowed to pass until it ceased to flow; upon which the canula was withdrawn. As the opening into the gut was too small, I passed a bistoury into the bowel through the previous incision, and commenced to enlarge it, directing the blade towards the sacrum. On effecting this, more meconium passed through the wound. A large, No. 10, elastic bougie was then passed, and subsequently a No. 12. Afterwards the parts were cleared, the bougie withdrawn, and the midwife was directed to bring the child to me at 9 o'clock on the following morning.

June 12. The child had passed a good night, meconium with fæcal matter had passed through the artificial anus, and it had made water. A No. 12 bougie was introduced, and retained for several minutes in the wound; on its withdrawal, a copious motion was voided. It had been fed on milk and water. A dose of castor oil was directed to be given to it.

June 13. 3 p.m. No fæces had passed since yesterday. The bougie was again introduced and a free flow of healthy fæces followed its withdrawal, giving apparently great relief. The infant had slept well, partaken freely of the breast-milk, and was now looking both well and good-tempered. Its urine was normal. Some more castor oil was ordered to be given.

June 14. The child had passed a free motion after taking the castor oil. A bougie was passed, and a free passage of fæces followed its withdrawal.

June 15. No fæces had been voided since yesterday. The bougie was introduced, and fæces passed afterwards. The child appeared well and happy, and so continued in every way until 9 o'clock, a.m., on June 20, when, although fæcal matter freely stained the bougie, no motion passed after its withdrawal. The same thing had occurred on the previous day. His abdomen was very much distended and looked blue. He appeared feverish, restless, and uneasy. The bougie was again introduced and retained a little longer. On its introduction it appeared to pass through hardened fæces; and, on its withdrawal, it was marked by fæcal matter. I ordered a free dose of castor oil and a warm bath.

June 21. Much better to-day, and has passed four or five fæcal evacuations. From this day he has not experienced an untoward symptom. From time to time I have continued to pass the bougie,

but beyond an occasional dose of castor oil, he has not required any medicine.

The mother stated some time ago that on his straining very hard to unload the bowels, she noticed two or three drops at a time of something like fæcal matter passed through the urethra, and that, except under these circumstances, his water is always perfectly clear. She states to-day, November 30, that since his birth she has noticed this circumstance half a dozen times.

November 30. The artificial anus appears to be firmly sound. The opening, which is now circular, looks like a sulcus, with its edges continuous with the external integument. The child seems to have control over his fæces; and his bowels act sometimes daily, and sometimes every other day.

February 4, 1869. The child is now seven and a half months old, a healthy, thriving, lively, unusually fat, big boy. Has cut his lower right central incisor tooth; and with the exception of slight disorders, incidental to teething, he has had no illness. The mother says that since last report no fæcal matter has passed through the urethra.

This is a case of complete atresia of the anus, and of about an inch and a half of the rectum, complicated, in all probability, with a fistulous communication between the rectum and bladder, indicated by the slight staining of the urine with fæces, as observed by the mother, and mentioned in the history of the case.

The success of this case I am inclined to think is largely due to the timely recourse to the operation before the child had begun to vomit, or, as is too commonly the case, had been worried by purgative medicines; to the little constitutional irritation produced by the operation, and the very slight disturbance occasioned in the position of parts, as compared with the method of M. Amussat, who draws down the gut to the perineal wound, and fixes it by sutures to the external cutaneous surface. The rectum was so firmly fixed that, by the bruising of the parts, which would have arisen from the force necessary to drag it externally so great a distance as an inch and a half, I should have run great risk, and I preferred incurring the hazard of subsequent coarctation of the wound; hoping, however, to prevent and overcome that difficulty by the regular and persevering use of bougies. The successful issue of the case has justified the method of procedure, which is, I think, entitled to preference as simpler and safer than that of M. Amussat. During the operation I did not perceive

any of the fibres of the sphincter ani, a circumstance unfavourable to the opinion of Roux, Petit, and Goyraud, as to the constant existence of that muscle; and supporting, as far as a single instance can go, the contrary opinion affirmed by Blondin, Züngel, and Velpeau, of its uniform non-existence whenever there is an absence of a portion of the rectum. I am, however, inclined to think that the absence or presence of this muscle is unessential, for Amussat observes that patients have been known to exercise control over their alvine evacuations even after excision of the rectum.

Finally, the success of this case, and that of others recorded in the pages of our own and foreign periodicals, should lead us to an opinion much more favourable than that expressed by Professor J. H. Bigelow, in the Boston Med. and Surg. Jour., vol. 57, p. 240; and quoted in Bodenhamer's valuable work on Malformations of the Rectum:—"Judging from results, I do not consider the operation for imperforate rectum, or even for imperforate anus, a desirable one. I believe that, in the present state of the art, it is better that a child born with either of those imperfections should die without this operation; although it must occasionally be performed in deference to established opinion."

ON THE RELATIVE VALUE OF SYMPTOMS IN THE DIAGNOSIS AND TREATMENT OF DISEASE.

By W. H. Day, M.D., M.R.C.P. Lond.,

Physician to the Samaritan Free Hospital, and to the Infirmary for Consumption and Diseases of the Chest, Margaret Street.

It will be my aim in this paper to refer, as plainly and concisely as I can, to some features in the course and progress of disease, as it falls under the notice of the practical physician. In any attempt to give a complete solution of this question, I feel that the highest powers of observation would be required on which to base any inquiry that should be profitable. Such a subject as the one I have selected might be pursued indefinitely, if we were to consider the nature of symptoms in all the various diseases we are called upon to treat; but, in a communication of this kind, it would be altogether out of the question. I only wish to point out some prevailing errors which lead to a mistake in the practical interpretation of symptoms. The great masters of the healing art in past ages dealt with disease by the cultivated faculty of observation and careful thought alone. If they had not for their guide and assistance, as we have now, the accumulated experience of centuries, they had not a thousand theories to disturb and perplex them. They saw disease naked and unveiled; and although they knew little of the nature of it, they did not overlook the importance and value of each symptom. Even the writings of our great physicians and surgeons of fifty years ago attest the fact, that they accurately studied and compared every sign and indication of disease, and their description of the course and symptoms of the diseases then known remain to this day models of the clearest observation. Disease shows itself to us by what are called symptoms, and on the due interpretation of these depends the power of diagnosis, on

which may be said to rest the success or failure of all treatment. It is the power which enables us to discover and find out a disease, to analyse and compare symptoms, to select remedies, and frequently to foretell the issue of the case. With it we have a lamp to guide us amid surrounding darkness, without it we are in total gloom and are the veriest empirics. By a symptom is to be understood any affection or change perceptible to the patient or those about him, whereby the body or its functions indicate a departure from health. Experience has taught us, that each individual organ or member of the body gives evidence of its derangement by certain signs peculiar to itself. The broad line of distinction which we draw between healthy and unhealthy action (which is only too apparent in the vast majority of cases that come under observation) is often obscure and ill defined. Where health ends and disease begins, or where the one merges into the other, more evidence is often required than is apparent to the senses; and until modern discovery assisted our investigations with the microscope, and other valuable means of obtaining information, we passed unheeded many forms and degrees of altered nutrition. Disease then escaped detection, till it attained a force and power, which struggled for ascendancy, sometimes acquiring it, and running with velocity varying with the strength or weakness of the organism. But even all the aids at our disposal for the detection of early morbid change do not expose the soil in which the essential germ of disease is first nourished. The patient, by his own sensations and feelings, is cognisant of something wrong before any symptoms are presented to the physician which he can pronounce positive and trustworthy. Yet, all evidence observed only through the medium of the patient is rendered untrustworthy, inasmuch as the same facts will be differently reported by different individuals. Hence the physician is constantly in the dark with disease till positive symptoms have made their appearance, or there is change in gesture, voice, or aspect, not clearly apparent to the patient himself. Guess work and chance, with the faith that time would work cures, have caused scepticism to creep in among us, and a distrust of all therapeutic agents. It is only when we confront disease that we realise our insecurity to interpret it, and to reduce it to anything like order and exactness. The classification of symptoms in our minds, is a great help to this object. I do not wish, in this paper, so much to give an exhaustive classification, as to point out several ways in which a particular class of symptoms receives ordinarily too little

or too much attention—thus, pain, which is a prominent feature in such diseases as peritonitis and pleurisy, may be valueless when affected by the personality of the patient. What one person will call severe pain, another will attach little importance to, and our inference from the patient's own report, without other attendant symptoms, should not weigh for much. Nothing is more common than to hear people say they have suffered the greatest agony during a whole night, and have not closed their eyes, when it is quite certain that, if it were true, such wakefulness and suffering must have told very decidedly on the general condition of the patient. Age, sex, constitution, and temperament have a wonderful influence in this respect. I apprehend that disordered sensation, having especial reference to the nervous system, is of more serious import than many forms of actual pain. When a patient, advancing in years, complains that his memory is less retentive than formerly, that he is nervous and irritable, that his limbs have in some degree lost their strength, our attention is at once directed to the state of the brain. Death from pain in the abstract is almost unknown; that it has occurred, and may occur again in a very delicate organisation, no practical observer will deny; the pain, acting like strong emotional feeling, is sufficient to paralyse the great nervous centre of the heart, and to arrest its action. The degree of inflammation will generally determine the severity of the symptoms, by which we judge more than the patient can tell us, of his state, and of his chance of recovery. Now the chances of recovery from severe illness are diminished in proportion to there being no well ascertained causes for his serious condition. If he is comatose and insensible from an overdose of fever poison, or if he has sustained any sudden and severe shock from accident or disease, he may have force enough inherent in his constitution to overcome these deadly influences; but a habit of body, always supported at a high state of tension, where the supply and expenditure are excessive, readily succumbs if any untoward event occurs to disturb or further shake the system. Organic life, as distinct from the inorganic world, is so difficult of explanation, that depraved vital action may be going on steadily, and working gradual changes in the different tissues of the body, which no means at our disposal can detect. A general and systematic survey of the complicated structure of the human organism, affords abundant evidence of the great mystery involved in the organic processes.

Symptoms are also applicable to morbid changes in parts which

the sight cannot reach. A symptom referable to an internal organ usually claims more notice than a symptom that can be referred to an external part, since it is clear that abscess, tumour, or inflammation are of much greater importance internally than externally. Looking at symptoms from a medical point of view altogether, we come to regard internal symptoms as more obscure than external. Defined and limited in their locality at first, they spread by extension to contiguous parts, and soon the original character of the symptom is lost. Every practical man is alive to the advantage of seeing a case early, to watch its progress and its caprice, and his opinion of the nature of the case may be much influenced by the stage at which he sees it. Symptoms are variable in their character and importance; some attract notice and consideration at once, others are known to be of trifling value. We do not underrate the gravity of severe pain, or its abrupt cessation, a rapid or very slow pulse, delirium, convulsions, hæmorrhage, diarrhœa, and so forth. I suspect that in many cases of sudden and unexpected death, we have not put the proper estimate on some one or more prevailing symptom—it has escaped observation. We are too rash in arriving at conclusions as to the cause of the fatal issue; and post-mortem examination has failed over and over again to throw any light on the subject. In this vain search for an evident cause where none exists, we have constantly ascribed the cessation of life to well ascertained causes of death, to disease of the lungs, heart, brain, &c., when it is quite consistent with what we know of disease to rest satisfied with the conviction, that death has resulted from the sheer failure of the organic processes to carry on the functions of life. The physical changes in the lung, in many fatal cases of pneumonia, prove beyond doubt that death did not result from ill aerated blood or asphyxia. When in fact death has to be so apprehended from these causes, such alarming symptoms have passed away, and the patient is restored to a condition which augurs only of safety; when, so far as his lungs are concerned, he has recovered, death has stepped in, and dissection has not cleared up the mystery. Those who have had much experience of lung disease will bear me out in saying, that the venous and leaden countenance of pneumonia, especially in young subjects, is not the grave symptom which some have described. I have seen it intensely marked in many cases, and pass away with attention to the general and constitutional symptoms alone. We see low pneumonia and congestion of the lungs set in when the system is

assailed with low typhoid, and the condition which has favoured this state of things is brought about through the nervous system. If, in such cases, the constitutional symptoms were more studied, and less regard paid to the local symptoms, we should not be so easily alarmed, for by raising the natural powers, by maintaining or endeavouring to maintain the strength of the circulation, we often overcome the evil of which I have been speaking. In combating disease, he is wisest who weighs the value of each symptom, and setting one against the other in the scale of importance, dissects out and treats the weightest. Whatever is done in phthisis, must be done through the stomach, and by attention to diet and constitutional symptoms.

The skilled physician, by looking too closely to physical changes, as if they were always the essence of the constitutional depression, has often overridden his mark; whilst the mere routinest, unacquainted with the meaning or signs of physical diagnosis, has, by a general view of the patient's condition, and an appeal to a common-sense view of the case, brought about a cure, when the other has pronounced it hopeless. If, in a diagnostic point, the physician has made no mistake, his very skill has led him into error.* I have known many instances where, the patient having given himself up after hearing that he had internal disease, the effect on the nervous system has been so depressing that it encouraged the progress of it; and it is manifest how different is the condition of a patient who hears a favourable opinion of his state, to that of an unfavourable one. We are beginning to allow that the nervous system plays a more important part in the production of disease, than was formerly supposed. Its influence on the blood and vessels, and its power to augment or depress the vital and chemical changes that are perpetually going on in the body, claim for it the first consideration.

It is very important that we should rightly estimate the value of

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* "Precision in diagnosis does not depend so much on the analysis of minute details, and on the detection of every trifling symptom, as on sagacity in eliminating all that is unimportant, and ability in selecting and giving due prominence to the salient points of a case. Success in practice probably depends as much on the development of this faculty, as on any single cause. In some individuals of great acquirements, their very store of learning becomes an incumbrance from want of tact in using it."—Mr. F. Le Gros Clark. "*Lectures on the Principles of Surgical Diagnosis*," *Brit. Med. Journal*, *May* 11, 1868.

local and constitutional symptoms; the former mark the changes that are going on in the affected organ, the latter indicate the effect such changes have wrought on the general powers of the system. It is deeply interesting to observe the marked differences that are produced in different cases. We are accustomed to speak of these two classes of symptoms as allied and inseparable; but in actual practice we see such a notion does not invariably hold good. Local changes are sometimes rapid and alarming, yet the constitution remains free; partly from the situation of the local mischief, but mainly from the vigour of the general health, and a natural resistance to overcome the endless circumstances in operation to produce disease. Now it is the constitutional derangement that induces us to search for a cause that may have disturbed and upset the general health; and if that cause is not discovered, we are driven to the position of ascribing the constitutional symptoms to delicacy of the general health; and our anxiety is not so readily awakened as when we have ascertained, by some of the means at our disposal, that a change is going on in some one or other organ of the body. I shall dwell at some length on this point, and further explain my meaning by the insertion of two detailed cases. I strongly suspect that in very many cases, the discovery of local lesions has attracted too much notice and attention, and that, by a too exclusive adoption of local measures, we have fostered and encouraged the constitutional excitement. It is quite certain that in many diseases, the general symptoms have only to be met, that the local symptoms may run their course in safety. For instance, from what we know of pneumonia, and judging from the condition of the lung tissue in its severe forms, we should think it a disease especially adapted for local treatment; but it is not so. And while I would not for a moment convey the idea that every case is to be treated alike, each case must be treated on its own merits, experience justifies me in saying, that constitutional treatment must not be lost sight of throughout the affection. Like almost all inflammatory affections it has generally a constitutional origin, and is more prone to seize hold of the weak and infirm than those whose circulation is strong and vigorous, and who are, so to speak, proof against the many slight causes in operation to produce disease in delicate constitutions. In the few cases of pneumonia happening to robust and strong subjects in which venesection has been successfully practised, we appeal to the constitutional before we can make any decided impression on the local symptoms. When this

is accomplished, the disease is often subdued before it has reached any degree of severity. There is a period or crisis with the local symptoms in this affection, which being allowed to pass by, the disease is no longer under the control of such constitutional treatment as I have named. All depressing measures, all remedies that lower the heart's action and control the circulation, must be employed early to be of benefit. This stage once passed, the constitution is unable to stand the shock of lowering agents, and as the disease advances, the remedy that might have stayed the force of the blood current, and prevented the engorgement and clogging of the air cells, and the consolidation of the lungs, would now jeopardise the patient's life. Now is the period to take up that which we have not succeeded in arresting, and thus to the constitution at last we look for help in removing the inflammatory products. The following case illustrates the importance of looking to the constitutional rather than to the local symptoms.

Case 1.—Recently I had under my care a case of common catarrh, which ran into bronchitis, and proved extremely obstinate. The attack was almost entirely confined to the large bronchial tubes, and the pulse never exceeded 100 beats per minute. The temperature of the room ranged between 70° and 80°, and there was always a gentle moisture on the skin. Here the most obstinate symptom was the cough, and the usual remedies, sinapisms, inhalations, expectorants, alteratives, and sedatives, gave no relief. The sputa, at first rather thick, became tenacious and frothy, and there was congestion of the lower part of the right lung. Crepitation was smaller, and threatened to extend to the minuter ramifications of the air passages. The urine was thick, acid, and high coloured. Next day the cough was worse, and the expectotation more frothy. Two days later the report states that a friction sound was heard, with slight effusion into the right pleural cavity. There was great pain. The pulse was 92, and weak, the respirations 24. There was headache, and the cough was harassing, day and night. The urine, on standing, was very clouded and almost milky; it contained an abundance of lithate of soda, and the deposit of albumen was considerable. It had an acid reaction, and the sp. grav. was as high as 10·25. This high sp. grav., and the absence of every trace of ædema in the extremities, seemed to point to active congestion of the kidneys, and to favour the supposition that it might tend to keep up the bronchial mischief. In spite of everything that could be devised, the patient continued

in a stationary condition; and depression of spirits, flatulence, and constipation were added to the list of symptoms. The cough was more than ever distressing, though the amount of sputa expectorated was small and thick. When this was got rid of by a spasmodic fit of coughing (which often terminated in sickness) the patient coughed up a clear looking fluid, which did not indicate any inflammatory mischief in the chest, but rather the result of excessive secretion on the part of the bronchial mucous membrane, which had become weakened and disposed to secrete largely. I determined to give brandy in large quantities during the day, and to avoid everything in the shape of medicine that could in any way depress. I gave nitro-muriatic acid, with gentian, chloric æther, and senega, although our stereotyped views of the general principles of medicine would scarcely warrant such a course of treatment. From this time the patient began to mend, the lungs gradually returned to a healthy condition, the expectoration was only trifling in the morning, and the urine became less thick and albuminous. In the course of a week she was up and following her duties, after a confinement to her room of six weeks. I do not mean to convey the impression that local symptoms are to be disregarded here, and that looking to the lungs, liver, and digestive tract, we were not doing all that was consistent with the present condition of medicine, seeing how bronchitis and other chest affections are kept up by flatulence, congestion of the liver and kidneys, and a loaded state of the bowels, or by retention in the blood of noxious elements, or other sources of irritation which ought to be separated from it. It is most consistent with our notions of disease to endeavour to rectify any functional disturbance that may exist. In this instance, and it pertains to many other cases, as soon as the constitutional symptoms received the largest share of attention, and the circulation improved in power and force, the relics of the disease were thrown aside. Disease does not usually show itself by any single or solitary symptom. I would even go so far as to say it never does; but when one symptom stands out in bolder relief than another, that symptom should be regarded with more suspicion than a multitude of minor symptoms. Much will depend on the precise nature of individual symptoms, and the organ to which they are referred. Prominent symptoms so impress the mind, that their absence indicates the absence of a disease, or of some internal and serious complication. Among isolated or prominent symptoms that stand out in bold relief as indicative of disturbance in the cerebral circu-

lation, is headache, if it is clear that it has its origin in the brain. It is a symptom which should never be overlooked, and it is of especial importance in the young and the old. When it attends slight disorder, and can be traced to sluggish liver, or intestinal disturbance it cannot be estimated of great moment. But the chronic and heavy headache of Bright's disease, and the confused and irritable headache of cerebral softening are well known.* Cases are on record in which the convolutions of the brain have been flattened, and tumours have been discovered, without giving rise to headache or any brain symptoms during life. This seems difficult of explanation, but two views appear to suggest themselves:—

1. The morbid growth is so gradual in its formation, that the yielding elasticity of the brain accommodates itself to the structure, and steadily advances in growth with it, so that it becomes as it were a part of the brain proper, and nourished by the same set of vessels and nerves.

2. As the functions of the healthy brain in man are more torpid in some than in others, so we may infer that a morbid growth or tumour is less an irritant in a person whose nervous system is less sensitive and highly endowed.

No doubt tumours and growths of various size and form exist in the brains of many persons, and when they do not interfere with the course or function of any nerve or vessel, when in fact they repose in the centre of one of the lobes of the cerebrum, they are not discovered till a post-mortem is made, or so long as they remain quiescent. It may, however, be relied on as a rule, that slow inflammatory action, the growth of tumours, or gradual softening

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* Lately a case of hepatitis and jaundice has come before me, occasioned by the passing of gall stones and biliary gravel, in which frontal headache and drowsiness were very marked. The symptoms were relieved by the free evacuation of bile and copious purgation. In one fatal case, the comatose state into which the patient passed, no doubt arose from the brain being poisoned through the infection of the blood, as happens in cases of yellow fever. The nervous functions are variously liable to derangements in different persons, and very slight disorder, or the mere alarm or agitation which disease occasions in some, may be enough to provoke symptoms which some morbid processes would fail to do. We are told that the injection of large quantities of bile into the blood of living animals is not followed by any deleterious effects. Such experiments do not conclusively solve important questions of this kind, seeing that the nervous system of the lower animals is less sensitive and impressible than that of man. His intellectual nature alone renders his nervous system more liable to disease.

of the cerebral mass, cannot go on without headache being an early and prominent symptom. We may have other symptoms to guide us, and we shall attach to them their proper weight and significance ; but freedom from headache is rare, and hence the value experience assigns to this symptom. The presence or absence of such a prominent symptom does not always make clear or certain to us the changes that are going on in the cerebral hemispheres ; nor does pathology always clear up our doubts and difficulties. There may be various degrees of alteration, giving rise to a precisely similar set of symptoms, and those symptoms may be most wanting on which we have built up our diagnosis, and anticipated recovery. The following case of remittent fever illustrates the absence of headache in a serious condition of the brain.

Case 2.—In July, 1861, I was called to attend a young lady, aged 12 years, suffering from what is usually termed a mild attack of infantile remittent fever. The pulse gradually fell in frequency, the skin became cool, the bowels regular, and the morning and evening exacerbations of fever gradually diminished. Suddenly without headache or any other apparent cause, when she seemed to be convalescent, the most alarming symptoms set in, and she died at the end of two days. A post-mortem examination revealed general inflammation of the membranes and superficial parts of the brain, with effusion into the lateral ventricles. It must be admitted as a rare occurrence for such serious disease to be going on in the centre of the nervous system, without causing premonitory symptoms. A little drooping and twitching of the right eyelid had been noticed a day or two previously to the attack, but there was no vomiting, dimness of vision, nor headache. Any degree of headache associated with this apparently insignificant symptom would have rendered it valuable, and a comparison of its worth would have instantly arisen. I make no doubt whatever that this was a grave symptom, and the sequel proves that the irritation within the brain was involving the third nerve at its origin, and not within the orbit as might have been supposed from the absence of all cerebral symptoms. I have noticed the same symptom many times, without headache, and watched its departure under aperient and corrective medicine. The difference between mere passing functional disorder and functional derangement is not always apparent; the latter arising from undetermined phenomena frequently passes into organic change. Our art nowhere shows its feebleness more than in this trying position. To further illustrate the absence

of headache in cases of cerebral disease, I may mention here that two cases of cerebral softening which I have recorded were unattended by headache.* In both cases the pupil of the affected eye was dilated, and objects appeared confused; the eyelid drooped, and the ball of the eye was drawn outwards. From these symptoms it was certain the third cranial nerve was affected in some part of its course. The absence of headache inclined us to the opinion, in one of the cases, that the symptoms arose from an affection of the nerve within the orbit; and an improvement in vision, and a diminution in the size of the pupil appeared to justify the diagnosis. These hopes gradually vanished, and the patient ultimately became paralytic, and died apoplectic at the end of two years. The second case improved whilst under treatment, but I have since lost sight of the patient. Here we have two striking instances of temporary amendment whilst the brain tissue was slowly undergoing disorganization. The only explanation that can be offered is, that if slight effusion exists it is absorbed, or the nerve fibres are renewed for a time.

Finally, the points I have tried to establish are—

1. That trifling symptoms are too apt to be overlooked, which may have a more important relation to the disease under which the patient suffers, than some more prominent symptoms which arrest the attention and are more peculiarly characteristic.

2. That we should look at symptoms collectively, and assign to each its true value, in relation not only to its prominence, but to the position of the affected organ.

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* "*Medical Press and Circular*," April, 1868.

DIPHTHERITIC PARALYSIS, AND ITS TREATMENT BY STRYCHNIA.

By Henry Maund, M.D., M.R.C.S.

Everyone who has seen much of diphtheria must have been much struck with the very peculiar nervous affections which are apt to follow it. The most common being paralysis of the soft palate, with a nasal tone of voice and difficult deglutition, liquids returning through the nose. Next in frequency is impaired vision, sometimes with strabismus; then loss of taste and smell; also loss or impairment of the power of motion and sensation of the lower extremities, and more rarely of the upper extremities. I have never known a case where the power of emptying the bladder or rectum was much impaired; in one of my cases there was total loss of sensation of the genitals, but still the power over the bladder and rectum was intact. My experience is that the frequency of these paralytic affections is in inverse ratio to the severity of the attack of diphtheria; I never remember to have seen more than one case where these secondary nerve affections followed a severe attack of diphtheria, though I have found them common enough after slight cases. I have met with several cases of secondary nerve affection where the patient had never been under treatment at all for diphtheria; but on close inquiry I have usually found that there has been "sore throat" a fortnight or three weeks before the paralytic symptoms first came on. Still further, I have seen cases of nerve affection, where the closest inquiry could elicit no history of the primary disease, but which, I am quite certain, were of diphtheritic origin. In the case I am about to relate in detail, the primary disease was so slight as to have escaped the memory of the patient, and yet how severe the secondary affection.

My experience of diphtheria is chiefly derived from cases which occurred in East Kent in the years 1857 and 1858, when it was

epidemic in a very severe and fatal form; but out of 200 cases attended at that time, not more than ten exhibited any secondary paralytic affection.

The Isle of Wight seems to have escaped anything like a severe epidemic form of the disease; during the four years I have resided there, I have attended nine cases of diphtheria, four of which have been followed by secondary nervous symptoms.

I have always found strychnia useful in these cases; but as most of my cases have occurred in children, I could not obtain the same satisfactory account of the effects which I obtained in the following case.

October 30, 1866, I was requested to visit a lady, aged 33, married, with two children, who was suffering with the following symptoms:—Severe frontal headache and giddiness; pulse 110; skin, hot and dry; total inability to sleep, though there was a feeling of drowsiness; pain in the forehead becomes worse towards night and better towards morning. I ordered saline aperient, and morphia at bedtime.

October 31. Much the same. The morphia produced some sleep. Frontal pain intense at 5 o'clock p.m.

November 1. No change. Intense frontal pain again at 5 o'clock p.m. Ordered three grains of quinia every four hours.

November 2. Pain came on as usual at 5 o'clock. Ordered four grains of quinia daily at 4 p.m.

For the next five days the periodic pain came on daily, but with less severity, and the febrile symptoms were diminished; still she did not consider herself much better. She had never suffered from ague, nor lived in a marshy locality. She stated that the pain came on two days before my first visit, while she was driving home from Newport to Sandown, just as she was crossing some low meadows enveloped in fog, and she was in her usual health at the time; beyond this, no previous history of the case could be obtained. She says that her sight is becoming dim, and that things seem to be in a mist. The urine was sp. gr. 1010, almost neutral, and highly albuminous. She suffered much from spasmodic pains over the cardiac region, feeling, when they came on, "as if she were going to die." For the next ten days she remained in much the same state; the sight was certainly more feeble on some days than on others. She states that she is also losing her taste. The urine became daily less albuminous, and by November 25th it was free from albumen, and remained

so during the whole of her illness. Her general health improved somewhat, and she left her bedroom, but her powers of sight and taste remained very feeble. I confess that I was puzzled with the case. For three days I had not visited her, but I was hastily summoned on December 1st, and found her in the following state:—She had suddenly lost the use of one leg; deglutition had been difficult for the last two days, and now the food returned through the nose. I again questioned her most closely as to any previous illness, especially sore throat. Her husband, who was present, said, "You complained of your throat about a fortnight before your first attack, and asked me to look at it." On questioning him as to the appearance of the throat, he stated that the tonsils were covered with spots which he could only compare to the "*whitish mould which comes on the top of soup which has been kept too long.*" She had used some camphorated brandy to the throat, and took no further notice of it, not regarding it of importance enough to require medical attendance. In two or three days it was quite well. The case was now perfectly clear. There was no doubt in my mind that the patient was suffering from the "sequelæ" of diphtheria. I ordered 1-20th of a grain of strychnia three times a day. For the next three weeks the paralytic symptoms increased.

December 21. Deglutition was performed with much difficulty, fluids frequently returning through the nose; the voice was so nasal that it was not easy to understand what she said. The sight was very dim; she could not much more than distinguish "men as trees walking." Her taste was completely gone, except for sweet things; she could not detect the bitter of the strychnia, but when syrup was added to the mixture she complained of the sweetness of it. The memory was much impaired. She had so far lost the use of both legs that she could neither walk nor stand. Sensation was nearly lost from the waist downwards; when placed in a sitting posture she could not feel that she was sitting on anything. She could just detect when the leg was pricked; but when pricked with compasses opened three inches, she could not tell that she was pricked with two points.

Sensation and motion were impaired, though in a less degree, in the upper extremities. She could not pick up small objects; the hands felt to her as if enveloped in thick gloves; she could give a fair squeeze with the hand, though it was some seconds before she could close the hand when asked to do so; she had great

difficulty in directing the hand towards a desired object. In the dark she was quite helpless, and could not move her hands, or tell in what part of the bed they were placed. If the hands were clasped, and the light removed, she could not unclasp them till the light was restored.

She states that she feels much better for a short time after taking the medicine. The dose of strychnia was being gradually increased, till, by the end of December, she took 1-10th of a grain four times daily.

January 1. The effect of the strychnia was now most remarkable; before taking her medicine she was almost blind, and could not read the large type of a posting bill. In half an hour after taking it she could read the leading article of the *Times*. The paper on the wall of her room, which looked to her, before taking the strychnia, of a uniform drab colour, gradually began to show the pattern, which in half an hour became quite distinct to her both in form and colour. The sense of taste returned, though in a less degree. The voice lost much of its nasal twang, and the power of swallowing returned perfectly. The whole aspect was cheerful and the mind active. A stranger who saw her before taking the strychnia, and again an hour afterwards, would scarcely have recognized her as the same person. She gradually relapsed into her former condition; the sight became dim, and she lay with an anxious and depressed expression of countenance, longing for the time to come for the next dose of medicine. The lower extremities were less affected by the strychnia, though, as she began to get better, it produced a feeling of warmth in place of the icy coldness which she usually felt from the knees downwards.

This treatment was continued for the next three months, during which time she gradually improved. The improvement occurred in the same order in which the powers re-appeared under the influence of the strychnia. The power of deglutition was restored first, the voice at the same time losing its nasal sound; then the sight returned; next the taste; then sensation and motion of the upper extremities; and lastly, of the legs. The restoration of some of these faculties, especially the sight, occurred suddenly. It was not till the end of March that she was sufficiently recovered to resume any of her ordinary duties. But she was still helpless in the dark; though she could walk from room to room in the light, yet when it was dark she could not put one foot before the other, or rise from her chair.

She could not be said to be restored to her ordinary state of health till the end of September, being eleven months from the commencement of the attack. She is now, if anything, in better health than she was before the attack. My friend, Dr. Park, of the Royal Artillery, saw the case several times with me, and was much interested in it, though I do not think he was quite convinced as to its diphtheritic origin, till he found a case related in the "Medico-Chirurgical Review," No. 73, p. 243, an almost exact counterpart of the one just related, the chief difference being that it followed a somewhat severe case of diphtheria, instead of a very slight one.

FRACTURE OF THE STERNUM FROM VIOLENT MUSCULAR CONTRACTION.

By Edward Beverley Bogg, M.D., M.R.C.P. Ed.

Fracture of the sternum is an injury of very rare occurrence, this fact being due not only to the shape, thickness, and situation of the bone, but also to the elasticity of the walls of the chest. Fracture of the sternum is generally transverse, and the lower portion of the bone may, or may not, be displaced. This injury has always been ascribed to mechanical violence *only*, but it may, in rare instances, be due to violent muscular contraction, as in the following case, which came under my own observation.

Thomas B——, a short spare man, was engaged in getting some casks out of the hold of a vessel, and while lifting one heavier than the rest, and exerting all his strength, he "felt something give way in his chest, and could scarcely breathe." He tried to go on with his work, but could not manage it.

When I saw him, his face was very anxious, and respiration both difficult and painful. On examination of the chest, I found that there was a notable and peculiar depression over the sternum. This depression was parabolic in form, the apex being immediately below the articulations of the fifth costal cartilages, and the base being at the lower extremity of the ensiform cartilage. The depressed portion was very mobile, and crepitus was distinctly audible. There was no bruising or discolouration of the integument. There were no symptoms of injury to the heart, or lungs, or to the internal mammary artery.

A broad bandage was applied, the man was sent to bed, and placed on slop diet. These measures gave him great relief, the dyspnœa only returning when the bandage got slack. He was kept in bed for three weeks, crepitus being inaudible after the fourteenth day; but he did not return to work till the forty-fifth day after the accident. The depression was then much

more shallow than at first. He continued under my observation for two years and a half subsequently to the injury, and his general health, which had been very good, became gradually deteriorated. He suffered greatly from dyspepsia, and became at last an ailing man.

The question then is, whether we shall say with Robinson, (Jacksonian Prize Essay, 1831,) that "Fracture of the sternum is *always* due to mechanical violence," or with De Lys, (Jacksonian Prize Essay, 1839,) that "The sternum may be broken by violent blows, by falls, by musket balls, and *other causes too numerous to mention*," including, under the latter head, violent muscular contraction?

Sir Astley Cooper has recorded one case in which the humerus, and another in which the tibia, was fractured by violent muscular contraction; and there is no assignable reason why other bones should not be *liable* to such an injury.

Chaussier relates a case where fracture of the sternum was occasioned by violent muscular contraction during labour.

The old treatment consisted in forcing the sufferer to take deep inspirations, under the hope that the depressed portion would regain its normal position; and when that plan did not succeed, the unhappy patient was fastened back-downwards on a barrel till the fracture re-united.

THE PHYSIOLOGICAL EFFECTS OF CHLOROFORM.

By Walter Whitehead, F.R.C.S. Ed., Surgeon to St. Mary's Hospital, Manchester.

It is now well known that, to render the brain unconscious of external impressions, it is only necessary to cut off or lessen the supply of oxygen to the blood. Upon this principle, and upon this principle alone, the *modus operandi* of all anæsthetics is now explained. To this theory I propose to take exception on behalf of chloroform and its immediate allies, and to show that there is another way in which chloroform may act as an anæsthetic.

The value of chloroform as an anæsthetic agent depends upon its effects on sensory nerves, and its paralysing influence on muscle. To speak definitely about the effects of chloroform on the nerves, it is requisite that we should know something about the *nervous agent;* and to understand the effect chloroform has in paralysing muscular power, we ought, in the absence of an exact, to have at least a comprehensive knowledge of the source of that power. I think we are perfectly accurate when we designate the nervous agent and the muscular power as the products of the changes food undergoes with oxygen. To deny this would be, as Bence Jones states, to deny the unity of nature, and to admit the creation of a power out of nothing. We know that food, after undergoing various processes in the alimentary canal, circulates as blood throughout the system, and receives from the atmosphere in its passage through the lungs a supply of oxygen, which it stores up in the cruorin, ready to supply the means of oxygenation whenever that chemical combination may be required. To admit of this combination between the pabulum on the one hand, and the oxygen on the other, it is required that some stimulus conveyed by the nervous agent should direct when and where that change is to take place, and whether it is to result in the development of

new structures, the renewal of old tissues, or the liberation of those conserved forces which constitute life with all its functions and sensations. The oxygen being as essential to all these changes and productions as it is to the combustion of coal and the heat and power the result of such combustion. If, therefore, all the functions of the body depend upon oxygen, this life-giving principle, it can readily be understood that to withhold oxygen is to arrest those functions, and consequently to suspend sensation. I presume it will be admitted that pain can be prevented in two different ways: either the brain can be rendered unconscious of external impressions, or the communication of sensibility from the periphery to the nervous centre can be interrupted. Now chloroform can induce either or both of these conditions. When a strong atmosphere of chloroform is inhaled, the brain is rendered unconscious of external impressions, simply because either the requisite amount of oxygen is withheld, producing asphyxia, or the muscle of the heart is suddenly paralysed by the process it is my intention immediately to suggest. But if you apply chloroform locally, or inhale it in dilute quantities, the sensibility of an excited part can be removed, yet the brain remain active to surrounding objects ; in fact, the impression is arrested between the excited part and the nerve centre. In consequence of this property of chloroform, the leg of a frog can be rendered insensible before the remainder of the animal is affected ; and owing to the same peculiarity Mr. Coleman was enabled to withdraw his own tooth under its influence without suffering pain. Now it is in this respect that chloroform and ether differ from almost all other anæsthetics. Nitrous oxide, carbonic acid, and other gases of a similar nature, beyond doubt derive their virtue as anæsthetics solely by interfering with the oxygenation of the blood ; and as might be expected, the condition induced by them, I mean the cadaverous countenance, strongly reminds the beholder of a state of asphyxia. But such agents act only by rendering the mind unconcious of external impressions, and, as far as I am aware, are not capable of destroying the sensibility of a part without first of all depriving the brain of all susceptibility. When chloroform is inhaled it permeates every tissue of the body, vascular and extra-vascular, and when eliminated from the body it is still chloroform, not undergoing in the system any change whatever. It is therefore upon its mere presence in the tissues that the effects of chloroform are dependent. That it displaces some of the oxygen conveyed by the cruorin of the

blood to minister to the chemical changes of the body, I can readily admit, but I must confess only to believe it to a very limited extent, and that only in the proportion in which it dilutes the oxygen in the inspired atmosphere. If chloroform rendered the cruorin totally incapable of carrying oxygen, all chemical work would instantly stop, and death would immediately follow. I am anxious to look for some better explanation of the phenomena of anæsthesia as produced by chloroform, and if I trespass beyond the limits of your credence, I have the satisfaction of knowing that I shall receive that gentleness of treatment at your hands, which must always be extended to the earnest worker in that vast field of mystery of which the pass word is "Unity of Research." To account for the effects of chloroform by the theory before alluded to, and which allow me by way of distinction to call the "oxygenation" theory, it is required, I may say necessary, that the chloroform be in the blood. But it can be shown that chloroform directed in a stream on to a pulsating heart at once arrests it movements; and it is also a fact that chloroform has only to come in contact with a nerve filament to suspend its sensibility. I think that this at any rate suggests a mode of operation distinct from and unconnected with the arrest of oxygenation. For the brain to be susceptible to an external impression, it is required that between the two an uninterrupted continuity of nerve tissue should exist. We know that it is only the central portion of a nerve which is possessed of the power of conveying the nervous agent; the coating round the axis-cylinder merely acting as an insulator, but nevertheless being most essential to the integrity of the nervous agent.

Whatever the nervous agent may be, Du Bois Reymond has established one of its properties which gives it identity, and we now know that its velocity is not more than thirty mètres in a second, whereas electricity travels at the rate of four hundred and sixty-four millions of mètres in a second. This fact, together with several other points of dissimilarity, is a barrier at present to our being able to prove the strict identity of electricity and the nervous agent. Still they have so many properties in common, that it is difficult not to see that there are connecting links which render their complete separation impossible. I may mention amongst others, that both require two dissimilar substances to excite their action, that both require to travel in a circle, and always in one direction, that both of them require insulation, and

neither will tolerate a solution of the conducting medium. Although, and this is one of the strongest arguments against their being identical, you may cut the wires conveying a telegraphic message, and by reuniting the ends recover the conducting power; yet when once a nerve is impaired by cutting its length in two, the transmission of impressions is destroyed, and the joining of the cut surfaces is of no avail. At any rate, whatever solution of this difficult problem may ultimately be arrived at, there is one property common to both the nervous agent and electricity which bears upon my subject, and upon which I am anxious to lay particular stress, that is the *necessity of a continuous insulated conductor*. The beautifully conceived hypothesis of Du Bois Reymond, that if the electromotive molecules of nerves are minute centres of chemical action, all arranged regularly so as to turn homologous sides the same way, mutually determining their position of equilibrium, and controlling their deviations from it; that, in such a system, though electricity were the connecting link of the whole and the means of transmitting power through it, the rate of transmission would be independent of that of electricity, and might indeed be what the rate of transmission of the nervous agent really is.

Whatever view is taken with respect to the nervous agent, and I think this view may as well be accepted in the absence of a better, because it has at least the recommendation of being in harmony with all the known laws of nature, one thing is equally true, the contiguous molecules composing the axis-cylinder of the nerve must always have a certain relation to one another; remove a single molecule, and we know that an entire chain of communication between the peripheric end and the nervous centre is destroyed.

After this brief but necessary digression, I come to the bearings of chloroform on my subject. It has been previously stated, that chloroform when inhaled permeates every structure of the body, and consequently must insinuate itself between the constituent molecules of the nervous substance, thus interfering with their mutual relation, upon which the fulfillment of their function has been shown to depend. For example, to introduce any insulating substance between two of a chain of pith balls would be to interrupt an electrical current passing through them; and in the same way to insinuate an insulator between two molecules of the nervous substance, would have the same effect as though you had cut the nerve in two. Now chloroform is proved

to be a bad conductor of electricity, consequently it must be a good insulator, as all bad conductors are in an inverse ratio insulators. Such being the case, chloroform has only to act as an insulator between the molecules of the axis-cylinder to impede the current of the nervous agent. Moreover, if each molecule can be proved to be a minute centre of chemical action, insulation would have the effect of exhausting the intrinsic value of each electricity-producing-point, and collectively of destroying the utility of the entire nerve as a means of communicating motion or sensation. Allow me to contrast the effects of chloroform with those of nitrous oxide, which may be taken as a typical example of an anæsthetic whose *rationale* can be explained by the circulation of unoxygenised blood. When nitrous oxide is inhaled you may expect after twenty to thirty seconds lividity of countenance, if not "a purple, ghastly, and frightfully death-like aspect;" after fifty seconds have elapsed the eye becomes unsteady, the hands slightly convulsed, and muscular rigidity either then occurs, or at the instant the operation is commenced. The moment you cease to administer the gas, the next gasp of pure air produces immediate restoration. The chain of events following the inhalation of chloroform are too familiar to repeat; but I must ask, is there such speedy recovery when chloroform is withdrawn and fresh air admitted?

When a stream of chloroform is directed on to an exposed pulsating heart, it instantly paralyses that muscle; whereas if a stream of nitrous oxide is directed in a similar manner, its effect is to stimulate, and cause quickened and spasmodic contractions of that organ. If it can be shown that the presence of chloroform in the blood prevents the absorption of oxygen from the inspired air, it could be proved that chloroform diluted with nitrogen would have the same effect as when diluted with air. In case such a mixture were compounded, I should anticipate the effects of the nitrogen long before the chloroform. Again, the effects of chloroform are not manifested with the rapidity which would naturally follow if it acted by depriving the blood of oxygen. Chloroform requires time to enter the remote tissues in the same manner in which it requires time to leave them. All the time chloroform is in contact with the remote tissue, however, the effects remain and the parts containing it are insensible.

It is almost unnecessary to draw attention to the fact that chloroform and ether alone have the power of producing muscular relaxation, and, in consequence of this negative property, of

dilating arteries, &c., by paralysing the coats of the arterial walls. Nitrous oxide on the contrary, by a stimulating action, causes contraction of muscle, and consequently a contracted condition of the vessels.

Although I must admit my view speculative, I have far from exhausted all the points of dissimilarity between deoxygenating anæsthetics and chloroform,—which probably acts as an insulator,—because they will readily present themselves to anyone who has given the subject of anæsthesia mature consideration, and the necessarily brief nature of this paper permits me only to suggest, not to demonstrate this mode of action.

AN EXAMINATION OF THE EFFECT OF RESIDENCE IN ALPINE REGIONS, AND OF DIFFERENT CLIMATES IN THE PREVENTION OR CURE OF CONSUMPTION.

By Charles Robert Drysdale, M.D., M.R.C.P., F.R.C.S.,
Physician to the North London Hospital for Consumption, Hampstead, &c. &c.

Before entering on the question of the treatment of consumption, we must of course endeavour to define what we mean by the term. Whether or not the word "fibroid," used by Dr. Andrew Clark, be a good one, it seems at present fully made out that consumption is a generic name, and that there are several species of that fatal disease. For example, I myself have, in more than one case, observed the occurrence of the disease as one of the *sequelæ*, as it would be termed by Mr. Hutchinson, *i.e.*, one of the tertiary forms, of syphilis, and in one case rapid and marked alleviation of the symptoms was caused by the administration of large doses of iodide of potassium. The consumption of drunkards is another variety of chronic pneumonia, well described by Huss and Richardson, examples of which I am constantly in the habit of thinking I perceive among the patients of the Metropolitan Free Hospital, many of whom are exceedingly addicted to spirits. As to the chronic pneumonia of the Sheffield grinders, of masons, and of cotton beaters, they are all as yet confounded under the common name of consumption, and it remains for a future generation to give them well-defined names, and thus render prognosis more certain. All of these forms differ materially from the ordinary cases of consumption, so frequently hereditary, and so often remarked among the debilitated populations of our large modern cities. Every medical man of much experience must have witnessed cases

of recovery occasionally among persons suffering from the former varieties of the disease; and two examples of the kind have recently come before my notice in the Hampstead Consumption Hospital, which were certainly very striking. With regard, however, to the last-named variety of consumption, namely, that wherein tubercles are well marked, it must, I fear, be admitted that the treatment of such cases is as yet most uncertain in its aims, and most disheartening in its results; some admirable observers, indeed, among others M. Villemin, asserting that the disease is uniformly fatal. This assertion, I believe, must be received with some qualifications, and I have myself certainly known of cases where I considered that the patient was decidedly tubercular, where years have passed away, and the patient has regained a certain degree of health and strength, and was likely to live some considerable number of years. In two cases at Hampstead, within the last two years, where patients carried out my advice to them to be out of doors all day in the open air, except at meal times, and to walk as much as possible without fatigue, an immense improvement took place, and they were enabled to resume their employments; that is, one was a labourer, and the other, at my advice, went to work in the country.

Partly, however, from the weakness of most of the patients, and partly from their own want of energy, it is very difficult to get them to remain long enough in the open air, even in such agreeable localities as Hampstead, although that spot is undoubtedly one of the most healthy and invigorating of any in the neighbourhood of London, and probably one of the best fitted for carrying out the hygienic treatment of pulmonary consumption. I confess myself to have but little faith in the pharmacopœia in treating phthisis, and this I say because I differ greatly in this opinion from some gentlemen of great experience. For example, Dr. Timms, in his work "On the Successful Treatment of Consumption," strongly advocates the administration of several powerful remedies, among others bichloride of mercury, as well as many other articles of the pharmacopœia, as frequently causing rapid amelioration of symptoms, and even eventual cure of the disease in many cases. His experience has certainly been very large, and he gives fully detailed cases; notwithstanding which evidence I remain sceptical as to the advantages to be derived from the pharmacopœia in cases of consumption. No doubt the oil of the cod-fish is contained in the codex; but as we know that, on an average, one ounce of fat is, or

ought to be, consumed daily by each of us, it seems but reasonable to suppose that this oil acts merely as butter, fat, &c. The perchloride of iron has been very greatly praised, especially by Dr. Cotton and by Dr. Jones, of the Metropolitan Free Hospital, as a curative agent in consumption; and I doubt not that it is advantageous in cases accompanied by great anæmia; but I cannot say that a trial of it, which I made for some months, led to any very striking results. It seems to me, too, that a large number of physicians at the present day are inclined to doubt the efficacy of any peculiar drug in the arrest of this fatal disease. Not that we cannot imagine that such a drug might be discovered. For my own part, since I have repeated the experiments of the great French discoverer, M. Villemin, of inoculating tubercle and sputum of the tuberculous upon rabbits with such astonishing results, I have often thought with him that there seemed to be something almost specific in the way in which tubercle, when it has once become deposited in the lungs, continues to ravage in all directions, apparently to a great extent by its poisonous properties in the economy; and when we have before us the remarkable example of iodide of potassium, with its power of arresting the spread of syphilitic ulcers even in the lungs, is it forbidden entirely to look forward to the time when perchance some salt or other remedy may be found able to arrest the progress of *some forms* of cancerous or tuberculous ulcerations ?

In default of all such actively curative agents, our present remedies are, I believe, entirely those which are placed within the domain of hygiene, and it is now my task to examine the evidence for the advantage said to be derived from change of climate, and especially from a residence in Alpine climates in the treatment of consumption. The remark has been long made by physicians and travellers, that consumption is much rarer in localities situated far above the level of the sea. Thus, Von Humboldt, in his "Notes of a Traveller," published as long ago as 1853, mentions that the town of Quito, 9,000 feet above the sea, as also that of Santa Fé de Bogota, somewhat of a like elevation, were both of them free from consumption. Holton, an American traveller, in a work entitled "New Granada," published in New York in 1857, asserts that the hospitals of Bogota do not contain a single consumptive patient. Dr. Smith, "Travels in Chili and Peru," who lived nine years in Peru, informs us that the Peruvians are so persuaded as to the immunity of elevated regions from tuberculosis, that they send their

patients into localities situated among the Sierras, upwards of 10,000 feet above the sea. A German doctor, Tschudi,* observes that phthisis, which is common in the low country and on the sea-shore, becomes very rare in proportion as we ascend the mountains, and that at 13,000 feet it is completely absent. Some physicians who have visited the highlands of Mexico, are quite as unanimous as to the rarity of consumption in certain altitudes, and that it is very rare at Mexico itself. Mr. Newton, writing on the "Medical Topography of Mexico" in 1848, states that consumption is very rare in that town, which is situated about 7,000 feet above the sea. A French physician, indeed, M. Jourdanet, seems quite enthusiastic as to the value of high climates in this disease. Writing in 1864 on "Mexico, its Climate, Hygiene, and Diseases," he says, "Le jour où les hommes le voudront, le ciel de l'Anahuac éteindra la tuberculisation du poumon." Although several of these works just cited are at variance with the results arrived at by other observers, and especially with those of Dr. Pravaz, who, in an "Essay on the Employment of Compressed Air,"† written in 1850, vaunts the advantages derived from compressed air, this question requires to be studied carefully. It must be remembered that consumption is still the greatest plague of the human race. In Europe with 266,000,000 of inhabitants, there is said to be annually a mortality of 931,000 from this cause, which gives about 3·60 deaths per thousand inhabitants annually. In France 150,000 are said annually to perish from phthisis; and perhaps about from 75,000 to 80,000 in this kingdom. Of course, it would be more important for us to know in the future, not whether consumptive persons are to be found in elevated regions, since such climates may perchance cut off all the weaker ones early; but whether or not those persons who have tubercles already are benefitted by being conveyed to such elevated situations. If so, the inhabitants of these islands might utilize the regions of the Himalayas, and other high mountain ranges, to the benefit of our numerous population of consumptives.

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* "Ueber die geographische Verbreitung der Krankheit in Peru" (Oesterr. Medic. Wochschft, 1846).

† Of the importance of the compressed-air bath, of three or four atmospheres, in the treatment of *asthma*, that is, in emphysema and bronchitis, I am fully aware. There is such a bath at Ben Rhydding Hydropathic Establishment, and a relative of mine derived immense benefit from a stay in it; but I cannot believe that it can be a curative agent in pulmonary consumption.

In ascending mountainous districts the air becomes rarefied, and an ascent of about 900 feet takes off about half-a-pound of weight from the surface. The physiological effects begin to be perceived when we ascend from 2000 to 3000 feet above sea level, in acceleration of the pulse and respiration, with increased evaporation from the surface and of watery vapour from the lungs. The digestion is benefitted, and the vigour of the nerves and muscles is said to be increased, in altitudes which are not excessive, such as from 4000 to 6000 feet. Jourdanet, it must be said, in his above cited work, says of Mexico, that "those who live in these elevated altitudes are feebler, more sickly, and seldom reach the natural term of existence." This assertion, however, has been denied by Coindet, a physician of Mexico. Every physician knows that a residence among mountains is most desirable in the treatment of diseases of debility. In fact, mountain air and sea breezes are proverbial in their influence upon the sickly and enfeebled dwellers in towns. Neuralgia, scrofula, and consumption have long been known, according to Parkes, in his work "On Hygiene," to be rare among the dwellers in high lands, and the curative effects of such places on such diseases is also marked; but it is possible, he adds, that the open-air life which is led has an influence, "as it is now known that great elevation is not necessary for the cure of phthisis." Phthisis is spread, then, over the whole globe, and yet is certainly rare on great heights. In Germany, whilst the disease is very frequent among the low-lying manufacturing towns, it is rarely met with in the mountains, as, for example, in the Hartz mountains and Thuringer Wald. Dr. Brockmann, ("Hirsch, Handbuch der Historisch-Geographischen Pathologie,") out of 80,000 patients treated by him in the Upper Hartz mountains, at a height of 2500 feet above the sea, had only twenty-three cases of phthisis, and nine of them had come with phthisis from afar. Müller (Hirsch, loco citato) states, that in Hesse, and in the Palatinate, where consumption is common, it is very rare indeed in the heights of the Taunus mountains. The same immunity is claimed by other observers in the mountains of Hungary, in the Carpathians, and in the valley of the Engadine. A distinguished observer, Lombard, in a work entitled "Climates and Mountains," (Paris, 1858,) says that—"If the low valleys and the middle altitudes of the Alps present a great number of phthisical patients, this kind of disease becomes rarer as we ascend the mountains, so that at 1000 to 1200 metres we only

meet with some isolated cases, and between 1200 and 1500 metres it completely disappears." The observations of Schnepp ("Influence des Lieux sur la Fréquence de la Phthisie, Communication à l'Academie des Sciences") in the Lower Pyrenees, prove the rarity of consumption in these districts. Hirsch states also that on the high plains of Abyssinia tuberculosis is unknown, and further that two observers, Wagner and Polack, have asserted that in the high plains of Armenia, phthisis is only observed in individuals coming there from more southern countries. The same observer, Hirsch, informs his readers, we know not with what truth, that in India, where consumption is very common, it is very rare, and even unknown, on the high plains of the Ghauts and Neilghery hills, at heights of from 4000 to 7000 feet, as also in the mountainous regions of Java; and that sanataria have been established for twenty-five years in Ceylon, Hindostan, and the Himalayas, at heights varying from 2000 to 3000 metres, *i.e.*, from 7000 to 10,000 feet. In the sanatorium of Sckin, he informs us, that Dr. Hooker had never met with phthisis. Guilbert, in a work published in 1862, in Paris, entitled, "Phthisis in Peru and Bolivia," mentions that this is the dominating disease of that country; but that there are in the Cordilleras, or Rocky mountains, towns of 10,000, 20,000 and 40,000 inhabitants, where no cases of consumption are met with, except among Creoles who have been sent thither for their health. Nicol, in ten years of practice, did not meet with a single case of consumption at La Paz, a town of 40,000 inhabitants, situated at 12,000 feet above sea level. Hirsch attributes this rarity of consumption in elevated localities to a special modification of the organs of circulation and respiration.

The writer, who in this country, seems to have paid most attention to this truly important branch of hygienic therapeutics, is Dr. H. Weber, of London. According to that gentleman, in his work "On the Swiss Alps," (1864,) tubercular consumption occurs not unfrequently in the lower mountainous or sub-Alpine districts. Of this fact I can assert the truth, having myself resided for some time in the canton of Grisons, where consumption is by no means uncommon. In the Alpine region Dr. Weber believes it is nearly absent. "It is," he says, "of very rare occurence among the monks of St. Bernard, and among the inhabitants of the Upper Engadine." When residing in Coire in the Grisons, in the neighbourhood of the Engadine, I have heard this asserted. What is more important, too, we hear from Dr. Weber of inhabitants of the

Engadine who have left their homes, contracted phthisis, but who, returning to their native valley, were cured of their disease in many cases. That gentleman gives some detailed cases of this kind in the "British Medical Journal," of 1867. In the tropical zone, consumption, according to these authors, may be regarded as rare above 7000 feet; in the warmer temperate zone above 3500 to 5000 feet; in the colder temperate zone above 1300 to 3000 feet elevation; and, in Switzerland, from 46° to 48° north latitude, the frequency of its occurrence diminishes over 3000 feet, whilst in the Hartz mountains it diminishes at from 1200 to 1400 feet above the level of the sea, at a latitude of from 50° to 52° north. There is a remark which, we fear, invalidates the force of much of the evidence adduced by Weber and others; and that is, it appears that in many places the Swiss women who live in the lower heights suffer greatly from consumption. They are, it appears, employed in-doors in making some manufactures in *small ill-ventilated rooms*. The men, who lead an open-air life, are said to be exempt. Does it not seem very likely after all that it is not elevation and rarefaction of the air at all that prevents or cures consumption, but simply plenty of fresh air and exercise?

And this brings me to the second head of my inquiry, namely, Is there any climate in particular which we should recommend for the treatment of consumption? Few nations have a right to speak so confidently as to the question of climate as the inhabitants of this kingdom. We have an experiment continually going on in different points of the surface of the globe, as to the effect of tropical and very cold climates on the occurrence of phthisis. In the "Army Medical Report" for 1866 we find the following facts, which seem to indicate that consumption is pretty general in all countries. Among the troops situated at home, in 1000 men, there were in the years between 1860-65, on an average 17·8 admissions for tubercular diseases, and 3 17 deaths in 1000. In Gibraltar, the average annual admissions for tubercular diseases were 7·4 per 1000, and there were 1·15 deaths annually. In Malta, there were annually 10·4 admissions, and 1·76 deaths from phthisis. In Canada, the admissions were only 8·6 per 1000 annually, and the deaths 1·71. In Bermuda, the annual admissions per 1000 for phthisis were 10·5, and there were 2·60 deaths per annum per 1000 living. In the West Indies, in the Windward and Leeward command, the average annual admissions per thousand men for phthisis were 10·0, and there were 1·57 deaths per 1000 from

1859 to 1865 among the white troops; whilst among the black troops the admissions actually amounted to 18·2, and the deaths to 6·52 in 1000 annually. In Jamaica, the average annual admission for phthisis was 10·1, and the deaths 1·75 in 1000; whilst among the black troops no less than 17·5 were admitted, and 5·43 per 1000 died tubercular. In Sierra Leone, at present, there appear only to be black troops. The admission of these black troops into hospital per 1000 was 16·2, and no less than 7·05 per 1000 annually died at Sierra Leone of tuberculosis. The same tale holds good for Gambia and the Gold Coast. St. Helena seems very healthy altogether, and has only an annual admission of 6·2, and a death rate of 1·82 from phthisis; and the Cape of Good Hope has about 9·0 admissions and 1·35 deaths annually per 1000 troops. In the island of Mauritius the average annual admission per 1000 troops for phthisis has been 10·7, with 2·19 deaths of late years. In Ceylon there has been a great deal of this fatal disease; no less than an average of 16·8 admissions annually per 1000 men into hospital, and 3·52 deaths among the white troops; whilst among the black troops there seem only to have been 2·9 admissions and 1·26 deaths annually per 1000 from phthisis in the years between 1859 and 1865. In Australia and Tasmania the average annual admissions rise to 16·2 per 1000, with 5·29 deaths; and in New Zealand it falls to 8·7 admissions and 1·90 deaths per thousand annually. In China the admissions are 9·8, and the deaths from consumption 2·24 per 1,000 of the white troops; whilst among the native or Asiatic troops there are only 3·2 admissions and 1·08 deaths annually per thousand from this disease. In Japan, in 1866, there were 15·8 admissions into hospital per 1,000, with 2·25 deaths from consumption. In 1866 the average number of European non-commissioned officers and men serving in the Indian commands was 58,901. In the Bengal Presidency, the average admissions per 1000 of the troops for phthisis was 11·00, and there were 2·49 deaths per 1,000. In the Madras Presidency phthisis seems to have been rather more common, since the average admissions amount to 16·7 and the deaths to 2·00 per 1000 annually. In the Bombay Presidency, again, the annual admissions sink to 10·1 and the deaths to 1·88 per 1000 annually. What can we conclude from these statistics? Simply, I believe, that some local circumstances, and above all, I am inclined to think, the bad ventilation of the barracks inhabited by the troops, are the constant cause of the occurrence of tubercular

phthisis, and that climate has little or nothing to do with its production. Moisture, doubtless, also is a cause, although a minor one.

One of the most striking points in the history of this melancholy scourge of humanity is seen in comparing the present mortality from it in Jamaica, with what the mortality used to be in the "good old times;" *i.e.*, the days before scientific investigations had been made as to the cause of phthisis. "It is not fifty years," says Parkes, ("Hygiene,") "since the usual time for the disappearance of a regiment of 1000 strong was five years." The statistics in the West Indies can now show a degree of salubrity almost equalling, in some cases surpassing, that of home service. Tulloch states that in a barrack at Tobago, in 1826, the superficial space per man was 22½ feet, and there were only 250 cubic feet per man. The air was fœtid in the highest degree. There was more consumption at that time in Jamaica among the troops, than at home, and far more than in Canada. In former days the immense mortality was attributed to climate; may we not say the same probably for consumption? It is not a disease of any particular climate, but rather is caused in all climates and all localities by life in-doors and unhealthy occupations. We are, inclined, in many respects, to agree with the learned Dr. McCormac in attributing the mass of cases of pulmonary consumption to the vitiation of the air caused by the different employments of townsmen. And perhaps the best evidence ever given of the truth of this assertion is to be found in the admirable address on "Public Health," delivered by the venerable Professor Christison, in Edinburgh, in 1863. Consumption, it appears, accounts for 11·5 per cent. of the total mortality of Scotland, or 2·37 deaths in 1000 annually occur from that disease. The proportion, in country districts, has been long known to fall short of the mean, and in towns to exceed the average; and the difference in favour of the country parts of Scotland seems conversely to increase in a greater ratio than the diminution of the general mortality, and, other things being equal, always in an increasing ratio, according to the degree of rurality of the country district. Taking the population of Scotland in 1855 at three millions, the total mortality in 1000 was 20·80. Dividing Scotland into, large towns of 10,000 people and upwards, and rural mainland, comprising all smaller towns with the pure country, it appears, that the mortality from all diseases for the rural mainland, in 1855, was 18·00 in 1000,

and in the towns 25·80 in 1000. But the mortality from consumption was, in the rural mainland 1·86 in 1000, and in the great towns 3·33 in 1000.

" In Glasgow, whose population in 1855 amounted to 365,000, and where all causes of town mortality greatly abound, so that the annual death rate is 28·90 per 1,000, that from consumption is as high as 3·85 in 1,000. Edinburgh and Leith, with a population of 206,000, present a mortality not much inferior, viz., 23·80 in 1000; but there is a greater difference in the deaths from consumption, which are 2·83 in 1000. Contrast, however, with even the latter proportions, the data derived from the very rural counties of Caithness, Sutherland, Ross, Cromarty, and Inverness, comprising a population of 240,000, and we find that the general mortality falls to 16·17 per 1000, and that from consumption to 1·79. The consumptive mortality is already less than half that of Glasgow. But these Celtic mountainous counties are not so favourably circumstanced as other rural counties with respect to other sanitary influences, such as climate, food, and medical aid. Turn, then, to the agricultural lowlands of Scotland. In the fine agricultural counties of Roxburghshire, Peebles, Selkirk, and Haddingtonshire, if we exclude two small towns, Haddington and Hawick, (which, though under the town standard of the Register, 10,000, own to the high mortality of 1 in 40,) there is a population of 97,000, in which the total mortality sinks to 1 in 65, or 15·46 in 1000, and the deaths from consumption to 1,38 in 1000. In Fife, deducting 25,000 inhabitants of two unfavourably circumstanced towns, Dunfermline and Kirkaldy, the population amounts to 130,000; and here the general mortality is 17·50 in 1000, and the deaths from consumption 1·25 in 1000—only one-third of the proportion in Glasgow. In the county of Berwickshire we have the most perfect example in Scotland of a population combining the richest agriculture with freedom from the deteriorating influences of mining, manufactures, and large towns. None of its towns contain above 3500 inhabitants; there is, I think, only one large factory in it—a paper manufactory—and there are no mines. Here, accordingly, the total deaths in 1000 fall to 14·10, and the deaths from consumption to 1·04. The general mortality is half that of Glasgow, and the share contributed by consumption is nearly one-fourth of the proportion in that city. In a first-class town, such as Glasgow, tubercular diseases account for twenty per cent. of the mortality; in an agricultural county, such as Berwick-

shire, for eight per cent. only. In a given number of townspeople five die of tubercular diseases, for one in the same number of countrymen."

It has been the habit of legislators to look on epidemic disease as the chief cause of death in towns; but, henceforth, consumption must be looked upon as a more serious cause of destruction in towns, and Dr. Christison attributes the frequency of this curse of the race in towns, to the want of open-air exercise, in this respect quite adopting the opinions of Dr. McCormac, Dr. Parkes, and others. Looking to the immense extension of our large modern towns, and their depopulating effects on the rural districts, we have but little cause for congratulating ourselves on any marked improvement in hygiene. There appears to be a very great absence of phthisis in the native population of the island of Lewis. With a population of 8400 there were only four deaths from consumption in three years, according to Dr. McRae; and in Mull, according to Dr. McColl, a gentleman who had practised there for thirty-three years, the disease is almost unknown in a population of 12,000, among persons who had not left the island and resided in the larger towns. This is no mere tradition, but certified by able medical observers. There is no doubt that many trades followed in towns account for much of the mortality from consumption. Such trades are those of the grinders, stonemasons, and cotton operatives, and those who inhale fine dust, such as coal-whippers and masons; but it is very difficult to account for the vast mass of phthisis seen in towns, on such simple principles as that of their employments. Thouvenin, indeed, in an article upon the influence of several trades on the health, has arrived at the conclusion that, except the operations of cotton-beating, and dividing and carding of silk cocoons, of white lead, of grinding, and one or two others, industrial pursuits in general do not exercise any directly injurious effects on the health of the workmen. That author traces the causes of the deterioration of the health of the working classes of towns, and their greater mortality, to defects in their dwellings, to their hereditary dispositions, to skin diseases, and to venereal and tubercular diseases; to the excess of their premature labour, and to the insufficiency and bad quality of their diet; to the irregularity of their lives, especially at an age when their physical development is incomplete; and, lastly, to drunkenness. This summary of causes is, however, only another word for poverty, without even excepting that of hereditary predisposition,

since it has been shown by D'Espine, in the "Annales d'Hygiene" of 1830, that tubercular disease occasions 68 deaths in 1,000 among the rich, but more than 230 in 1,000 among the poor. I have found that in the healthy county of Hertfordshire, the annual mortality from consumption is 179 in 100,000 inhabitants; whilst in Liverpool it is 368 in the same number of inhabitants; 331 in 100,000 in Manchester; and as much as 402 in 100,000 annually in Merthyr Tydfil. In London the annual mortality from consumption is 277 in 100,000, or lower than that of Liverpool or Manchester; but nearly twice the mortality of Berwickshire. The annual mortality of all England from consumption is given by the Registrar-General's reports as 258 in 100,000, against 237 in Scotland. Another corroborative fact is mentioned by Dr. Livingstone in his "Travels in South Africa." He asserts that the natives there, although they seem often to be starved to death, yet do not suffer from consumption. This is a curious fact if it should be verified, and would tend to confirm the theory of Drs. McCormac, Parkes, and others. With respect to the greater frequency of death from consumption among the poorer than among the richer classes, it would indeed be surprising were these results of statistical inquiry to prove otherwise. Among many of the poor female operatives in large towns wages often do not seem to exceed sixpence a day. And in the sixth report of the medical officers of the Privy Council for 1863, I find that the report by Dr. Edward Smith, on the "Food of the Labouring Classes," states that the food of the silk-workers costs 2s. 2½d. a week; that of needlewomen 2s. 7d.; of kid-glovers 2s. 9d.; shoemakers 2s. 7½d.; stocking-weavers 2s. 6½d. The able author of this paper adds, "No class under inquiry exhibited a high degree of health. The least healthy are the kid glovers, needlewomen, and Spitalfields weavers. The average quantity of food was too little for health and strength." Of the needlewomen, he says, "This is the lowest paid class included in my inquiries. Their ordinary hours of work are ten to twelve hours. The average income was only 3s. 11¼d. weekly per adult." Readers of the works of Mr. J. S. Mill, Joseph Garnier, &c., will see in these statistics the unfortunate effects of the tendency of our race to increase more rapidly than it can obtain food and necessaries. "Nature," says the Rev. Professor Malthus, the great and illustrious discoverer of the "principle of population," which discovery I consider to be by far the most important in hygienic

science that has ever been made, "cannot be defeated in her purposes. The necessary mortality must come in some form or other; and the extirpation of one disease will only be the signal for the birth of another, perhaps more fatal. I believe that it is the intention of the Creator that the earth should be replenished; but certainly with a happy population, not an unhealthy, vicious, and miserable one." Misery and low wages cannot go on indefinitely. Consumption and fevers cut off the struggling operatives. Dr. Edward Smith, in a numerous set of inquiries put by him to 1,000 hospital patients affected with consumption, found that the *average* number of children which the parents of the unfortunate patients had produced was actually 7·5. No wonder that these poor people became consumptive in such unfortunate homes. We shall not be astonished after this that Mr. J. S. Mill says that "little improvement in morality can be expected, until the producing of large families is regarded with the same feelings as drunkenness, or any other physical excess."

The treatment, then, of consumption cannot be said to have made much advance since the days of Hippocrates. That wise physician recommended open-air exercise and milk diet in this disease; Aretaeus, sea voyages and good diet; Celsus and Galen recommended what I should now recommend, viz., country air, exercise, milk, and nourishing diet. Paracelsus, in the sixteenth century, and his numerous admirers since then, have tried mercury, antimony, &c., and done, of course, a great deal of harm by their experiments, which, alas! as yet have brought, we fear, no fruits in the cure of consumption. Perhaps, indeed, the only important addition made in modern times to the diet of the phthisical was the introduction, due in this country to Dr. Hughes Bennett, in 1853, of the oleum-morrhuæ,* and I cannot help believing that the hypophosphites, which I have often tried in vain, are utterly useless, and indeed inert. Besides which, this was the result of the Parisian Commission of the Academie de Médecine.

The effects of exercise, then, no doubt a highly complicated cause, and acting on both digestion and assimilation, and impure air, have been found, according to Parkes, &c., to be very potent agents in consumption; and, conversely, the conditions of preven-

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* The cod-liver oil called Möller's is excellent, and I am informed by my respected friend, Professor Boeck, of Christiana, that it is quite a genuine manufacture.

tion and treatment, which have seemed most useful, are nutritious food and proportionate great exercise in free and open air. "So important has the last condition proved to be, that it would appear that even considerable exposure to weather is better than keeping patients in close rooms, provided there be no bronchitis, or tendency to pneumonia or pleurisy." Persons who can afford to choose their climate, will find that from the commencement of October until the end of April, no climate can be more adapted for consumptive patients, with tendency to bronchitis, than that of Middle or Upper Egypt. Cairo and its neighbourhood is a delightful winter climate; its mean temperature in January is 59° Fahrenheit. After April, the patient, with power and means at disposal, should move northwards to Malaga or Mentone, which last-named place has been rendered classic ground by the writings of the eminent Dr. Henry Bennet. Dr. Prosser James has himself experienced much benefit from St. Remo, and greatly recommends it as a winter climate. To persons of the labouring classes, perhaps the best advice, when they can follow it, is to recommend emigration and then a country life. This, I feel convinced, would prove as efficacious as life in mountain districts, and my conclusion is that exercise and fresh air are at present the only means we have to contend against that greatest foe to human happiness, consumption; and that, whilst the evidence given for the residence of consumptives in Alpine districts seems very convincing, and requires further investigation, yet, that the main cause of the disease being in-door occupations and town life, the best prevention and cure of it is most naturally to be sought for in country life and out-door employments, with plenty of exercise and good nourishing food.

A REPORT ON THE PARASITIC THEORY OF DISEASE.

By Leonard W. Sedgwick, M.D., Honorary Secretary of the Association.

Old world fancies not seldom foreshadow new world facts. Ancient belief attributed many maladies to living things unnaturally present in the sick body, and modern pathologists say that they can separate the essence of more than one disease, and show it, by the aid of the optician, as a living, growing, independent organism.

For long this subtle subject has divided the learned of all nations. Some confidently asserting that very many of the disorders which afflict humanity are caused by minute beings, animal or vegetable, each producing its own sickness; others as confidently contending that these so-called living creatures are but delusions and phantoms. Some, again, admitting the reality of the forms, but denying their malignant influence; looking on them as accidental sequences rather than as essential causes of disease.

To examine the evidence on which these different opinions are based, and to attempt, in so far as I am able, a broad, general view of the influence of parasitic vegetation in the production of disease, is the purpose of the present Report. The subject is a vast one, and I approach it with many misgivings, and much questioning of my power to accomplish the task. I do not presume to think that I can present a complete view of the matter, which is developing from day to day, but I venture to hope that I may be enabled to suggest some subjects for profitable discussion.

At the outset it will be needful to obtain a definite standpoint by considering in brief some of the characteristics of the organisms in question, especially as to their mode of growth and their origin. And for this purpose it will be advantageous to speak of them under two heads. First, cryptogamic growths, of which the Yeast

plant is a type; and, second, minute structures, described by many as infusorial animalcula, of which the Vibriones and Bacteria of ordinary putrefaction are the commonest forms. I speak of them thus, separately, for the sake of convenience only, because, though differing much in structure and outward aspect, they differ little in essence and in action.

The lower orders of cryptogams require, as a rule, dampness for their free growth, and generally develop most freely in a warm temperature.

A single cell, with definite walls and often coloured contents, possessing the faculty of increase by segmentation, is the simplest form. In some kinds the cells, after division, live a separate existence; in others they remain more or less closely attached to each other; but, in both, the increase is the same in nature, and is purely a process of growth. The growth is not always obtained by division of the cell, sometimes the new cells are formed from buds or outgrowths from the old ones. The higher cryptogams are only a collection of these cells, modified in appearance by outward circumstances, as pressure, light, heat, food, and moisture.

In one or other of these ways the simple plant develops itself; for whether the cells be separated and wafted far and wide, or whether they be united for the whole of their life, development of this sort is still, to all intents and purposes, development of the same individual.

But another mode of increase exists, an increase which is not merely of the individual, but of the kind. It is produced by the conjunction, the union, and the intermixture of the contents, or a part of the contents, of two separate cells, which, when combined, form what is called a spore; this divides and subdivides, in its interior, until the common cell-wall is filled with minute cells, or nuclei, which burst their case, and then start into growth on their own account. Such is a true reproduction, analogous in its essence to the reproductive increase of all the higher organisms. This conjunction of two cells may happen, not only between cells which are free, but between cells which are confined by one common cell-wall, as in the Volvox. A true reproductive act of this kind has not been observed in all the lower cryptogams, for their life-history has, largely as yet, to be written; but it has been observed in so many that, knowing the uniformity of Nature's workings, it may fairly be predicted of all.

Such, in plain words, are the typical processes of growth and

reproduction in these plants; but the details are infinitely varied and infinitely complicated. It would be impossible, on the present occasion, and indeed needless for the present purpose, to describe what is known of these modifications; suffice it that I mention those things only which bear upon the matter in question.

Firstly, it may be said with confidence that the same plant has been described, at different periods of its growth, under different names. Forms which at one time were believed to be different species, are now clearly proved to be merely different stages of maturity of the same fungus; and it cannot be doubted, considering how little is known in detail of the reproduction and early growth of the cryptogams, that many more will be proved to be immature forms, and not different plants. Dr. Braxton Hicks has shown that growths, undistinguishable from the earliest protophytes, are produced from certain parts of mosses and lichens. The sori found on the surface of lichens, and composed of one or more green cells, with interlacing fibres, are structurally like their parent. These green cells closely resemble the Chlorococcus plant, and will divide and grow like it, for an indefinite period, under the same conditions.

The Yeast plant, again, for long described as a distinct species, is only one term of a series of forms which, in their sum, are the whole life of an individual. A minute sporule has found its way into a soil in which it can grow, some nitrogenous saccharine fluid for instance, and has developed into a simple, clear, nucleated cell: buds sprout from it which grow into cells like the parent, and, when in active growth, remain in connection with it; this is Torula. As the process of fermentation progresses, the tips of the growing points form chains of small round cells, which, dropping off, develop into the original form; this is Penicillium. In what by many is supposed to be a further development of the same plant, one or more large cells are produced at the end of the growing tips of the torular form, which fall off, germinate, and develop into filaments, which have, in time, large globular heads, called Sporangia. As these grow a multitude of minute spores are formed in their interior, which, when shed, have all the faculty of growth possessed by their parent.

Professor Hallier's recent observations on the growth of yeast fungi show a somewhat different series of changes. He says that, if the spores of mould, or blight, are put into a fermentable fluid, nuclei, which he calls cocci, are formed in their interior, which,

after the dissolution of the spore case, grow and multiply by fission. This is nucleated yeast, or Micrococcus, and is the source of all fermentations. By and bye a cell-wall is formed round these nuclei, multiplication by fission ceases, and they increase by buds, and form the ordinary yeast of vinous fermentation, which Hallier calls Cryptococcus. From this, or from the original Micrococcus, grows the fungus producing acetous fermentation, which he calls Arthrococcus; it begins by the protrusion of "five rod-like or quadrangular processes, or sprouts, which, when separated from the mother-cell, do not continue sprouting, but, like the micrococci, divide by partition." Professor Hallier admits that there may be other forms than these three, and describes some which he calls transitional; but his views are not yet generally accepted.

Secondly, it may, with equal confidence, be said that the same form will assume different aspects under different conditions. That a cryptogam grown in the dark is not like the same plant grown in the light; and that a plant fed on sugar is, in outward appearance, not at all like the same plant fed on nitrogenous matter.

Thirdly, that the sporular, or germ form of all these vegetable growths, is singularly resistant to destruction. These germs may be dried for years, they may be exposed to great heat and to extreme cold, they may be blown to and fro by every wind of heaven, and yet, when coming upon the conditions necessary for their growth, they immediately begin to propagate themselves. They are widely diffused in the air, are infinitely minute, and some of them grow in the most unlikely media; they may be found prospering in the pharmacopœial solution of alum and zinc sulphate, grow abundantly in ammonium tartrate and commercial sodium carbonate; nay even, according to Raulin, the Ascophora nigrans will grow in media consisting entirely of mineral matter; and the late observations of Mr. Roberts show that filaments of fungoid origin are to be found in flint.

The second form of organism which it will be necessary briefly to consider has been described by authors under the terms, Spirilla, Vibrio, Bacterium, and Bacteridium. They are very small in size, and very simple in structure—merely a definite boundary wall and a clear centre—they are round, or oval, or linear, or compressed in the middle, as if they had a waist. But simple as they seem to be in structure, their nature and mode of development have been, and still are, strenuously debated. They commence as granules,

which seem, even under the highest magnifying powers, to be mere points. These granules increase in size by ordinary growth, and increase in number by division in one axis. The division may be perfect, and the segments become separate and independent, or it may stop short of completeness, and the result may be a beaded body of greater or less length. The individual granules may, without segmentation, grow and become linear. Dr. Hughes Bennett describes the formation of bacteria as a process of approximation of neighbouring molecules, with more or less fusion of the individuals, at the point of contact; but the correctness of this is by no means generally admitted. These little beings have been divided and subdivided by different authors without, as it appears to me, sufficient warrant; for the external characters of many of the species are not sufficient to distinguish them from others, and recent observations seem to show that many of these differences may, as in the case of the higher cryptogams, depend solely on external influences, not on internal essence. Another source of confusion has arisen from the want of recognition of the fact, that motion is no more an evidence of life than it is of animality. Mr. Rainey has described the active movements of purely mineral molecules under certain conditions; and it is undoubted that dead molecular matter may exhibit motions which, in themselves, are indistinguishable from those of living organisms. Much uncertainty again is produced by the use of the words "infusoria" and "animalcula" in describing them. Time was when spermatozoa were believed to be living animals, separate existences, and the motion of their tails was held to be absolute proof of their individuality. In like manner these structures are spoken of as infusoria. With regard to a large number of so-called forms of vibriones and bacteria there is no justification for the imputation of animality. They have no digestive organs, and there is no evidence to show that they take up particles of matter into their interior and use them there as food. There is no differentiation, not even the most elementary, of organs; they are homogeneous throughout. Their motility differs in no degree from that of many of the confervæ; and even if it did, if it partook of the slow, deliberate, and apparently conscious movement of the Amœba, the recent observations of Dr. Braxton Hicks on the amœbiform movements of certain conditions of the Volvox, which is an undoubted vegetable, and those of Cohnheim and others, on similar motions of the white-blood cell, would deprive it of all value as a test of animality and

of individuality of nature. Again, they behave to chemical agents in the same way as do the lowest plants; and they are unacted on by potash, which destroys animalcula. This holds good in regard to a large number of so-called species; and whenever it does, it is proof, I contend, of their vegetable nature. But it is probable that it does not of all; and herein, I believe, is contained another source of confusion, for, after all, some may be really animals. The confusion to which I allude is due, I believe, to an imperfect recognition of the fact that, as yet, there is no proof that bacteria and vibriones ever complete their life as such. Indeed, there is much evidence to the contrary, and this is immensely strengthened, I think, by the absence of any distinct observation of their increase by the true reproductive act of conjunction or of intermixture. The mode of growth by adhesion, described by Dr. Hughes Bennett, might be an act of this nature; but this mode of growth is by no means generally admitted by those who have paid most attention to the subject, and no results, in the form of progeny, have yet been observed. Bechamp and Estor affirm that there are certain minute spherical molecules existing constantly in the human body, which, when exposed to air and moisture, grow into chains of bacteria; but this observation needs confirmation. Madame Lüders has, very recently, related some experiments which she seems to have conducted with the greatest care. She put boiled beef-tea into test tubes, which she heated to 160° C.; at the moment when they were removed from the heat, spores of fungi, taken up by means of forceps, previously heated to redness, were added, and the tubes hermetically closed. They were then placed in a warm bath, and, in twenty-four hours, vibriones, similar in all respects to those of ordinary putrefaction, were observed. At the same time a similar tube, similarly treated, with the exception of the addition of fungus spores, contained no vibriones. Experiments of the same nature have been performed by Professor Hensen with the same results. Madame Lüders even affirms that she has reversed the conditions, and developed yeast-cells from the vibriones of putrefaction when grown in a suitable medium. In investigations of this kind it would appear that much depends on the nature of the fluid and the range of the temperature; for spores, cultivated in water, would seem to have developed into rods without motion, which, when placed in flesh-water, multiply, obtain motility, and look just like vibriones which arise spontaneously. The latest observations of Professor Hallier tally with these, and

go to show that what are described as bacteria are, in many instances at least, the germs, or early stages of growth, of some form of fungus. Professor De Bary regards them as algal in nature. Hallier's terms, Arthrococcus and Micrococcus, include a large number, if not all, of the bodies described by other authors as bacteria.

Again, it would appear that the larval form of some of the lower infusoria is hardly to be distinguished from a Vibrio. In this way only is it, I think, possible to explain the observations of Pouchet and others, who describe the development of animalcula from bacteria and vibriones.

It would appear, then, that, under these terms, three different classes of objects have at times been described: dead particles of mineral or organic matter, and living structures, which, although usually vegetable growths, may not improbably, sometimes, be the germs of animals. The latter may be excluded from this consideration, and we may at once assume that all the organisms described under this head, as producing disease, are vegetables. But whence do they spring; how do they arise? The answer to these questions must rest on the answer to be given to that much wider and more debated question: Is there such a thing as Spontaneous Generation? This obscure but important matter cannot be fully discussed here, but, at the same time, it cannot be passed by in silence. A short summary of the contentions of the opposing parties, and some attempt at a critical estimate of their value, is necessary for the argument. The dispute is an old one, old as natural science itself; the one party contend that, in order that vibriones may form in any fluid, their germs must be introduced; the other, that vibriones, and even the lower infusoria, may arise in any fluid spontaneously. The one party believe that these organisms have always an ancestor of like nature to themselves; the other, that no such parent, and no parent of any kind, is needed. The experiments on which the opponents of spontaneous generation rely are typified by the following, related by M. Pasteur:—Three ounces of fresh urine were put into an eight-ounce bottle, boiled for some minutes, and then set aside to cool, in such a way that the air re-entered through a small red-hot tube of platinum. When the urine had become cold, the neck of the bottle was hermetically sealed, and it was placed in a stove at a temperature of 30° C. Six weeks afterwards it was opened, the urine was found unchanged in appearance, and no living organisms

were seen in it. A piece of asbestos, containing the dust of the air, was now introduced; in thirty-six hours putrefaction had commenced, and vibriones were present. In other cases the air, which had been allowed to re-enter the flasks containing the fluid, has been filtered through cotton wool, or the particles contained in it have been arrested, by compelling it to pass through contorted tubes; and in these cases, again, the appearance of vibriones has been delayed for long periods; while similar fluids, exposed at the same time to unfiltered air, have rapidly putrefied. A series of experiments by Professor Lister, conducted with the greatest care, and confirming Pasteur's, is related in the "British Medical Journal" of July 18th, 1869.

By these and such like experiments Pasteur and others have believed that they have demonstrated, that ordinary atmospheric air contains the germs of vibriones and bacteria, and that these germs are organised living corpuscles, not mere organic matter. On the other side many experiments are related, similar to these, and in which every precaution seems to have been taken, and yet vibriones have developed just as soon in the heated as in the non-heated bottles. Experiments of this nature have been made by M. Pouchet, Dr. Child, Dr. Hughes Bennett, and many others; but seldom has the result been uniform, and all have met with instances when putrefaction was immensely delayed. I cannot admit that facts such as these prove the doctrine of spontaneous generation. If organisms appeared in one, why not in all? If the doctrine were true, this is what should have happened. I confess that, to me, it seems much more probable that some germ-bearing air had accidentally found admission, than that such an irregularity as this should occur. Again, I do not think that the resistance of the germs of these bodies to heat has been sufficiently considered; Dr. Child only boiled his fluid ten or fifteen minutes, and other experimenters merely speak of having boiled for two or three minutes. But surely this is not sufficient. In the hot springs of certain Geysers unicellular plants have been found growing at a temperature of 93° C. All botanical works relate instances of seeds germinating after boiling, as in the making of jam. The Rev. J. Berkeley says that fungi spores are not destroyed by boiling-water. Payen has shown that the spores of the Oidium aurantiacum, scattered on bread and exposed to a temperature of 120° C., will still grow, and reproduce the fungi. Madame Lüders says that, in her experiments, heating to 100° C. is not

sufficient to kill the germs of vibriones, but that, when the fluids were heated to 160° C., no organisms then appeared. Again, Dr. Wyman reports, as the conclusion at which he has arrived from his numerous experiments, that solutions of organic matter, boiled twenty-five minutes and exposed only to air that had passed through red-hot tubes, became the seat of infusorial life; that similar solutions contained in flasks, hermetically sealed and then immersed in boiling-water for periods varying from a few minutes to a few hours, also became the seat of infusorial life; but that, if the boiling were continued beyond five hours, none appeared. Many other experiments are related, especially by French and German observers, but I think it must be admitted that there is still wanting conclusive proof that these lowest organisms arise from anything else than a living parent of similar nature, and that the reproductive law, which is universally admitted to be operative throughout almost the whole domain of Nature, gives place to another and an opposite one, in these her simplest and earliest children.

The demonstration of the existence of Heterogeny rests with its supporters; and in the face of the uncertain results which have followed the experiments related by them, this doctrine, as well as its modified form, ably advocated by Dr. Hughes Bennett, in a lecture delivered to the Royal College of Surgeons of Edinburgh, on January 17th, 1869, still waits for that support which can alone conclusively prove its truth, experiment, rigorous in its application and invariable in its result

Such, then, are the organisms with which we have to deal in the consideration of the parasitic theory of disease. If we would rightly estimate their power, their connection with the processes of putrefaction and fermentation must have some attention. But, again, I must rather seek to indicate the bearing of the known facts than to relate them at length. These processes, though they seem to demand at the outset the presence of oxygen, are not really processes of oxidation, but rather acts of elementary transmutation. Wherever fermentation and putrefaction are proceeding there vegetable growth is found; torulæ, or some allied form, in the one case, and vibriones, of some sort, in the other. And there is every reason to believe that these organisms are the essential causes of the processes. Air passed through red-hot tubes will not excite fermentation in a solution of sugar that has been boiled with yeast; fresh grape juice will remain for years unchanged, if

kept, over mercury, from contact with the air; and in the experiments which I have related, when discussing spontaneous generation, vibriones and bacteria have always been shown to be present when putrefaction was proceeding.

The recent observations of M. Van Tieghem on the fermentation of tannin have the same bearing. This substance, under certain circumstances, undergoes fermentation, and, along with water, is changed into gallic acid and glucose. According to this observer the presence of fungoid growth is essential; a growth which proceeds from spores contained in ordinary atmospheric air.

M. Pasteur asserts that the different fermentations—the vinous, the lactous, the acetous, &c —are produced by different species of fungi; but it may well happen that some of these are merely varieties of one species, altering its form and appearance in accordance with the different conditions under which it exists; indeed, Professor Hallier asserts as much. The fungi grow as long as they have food, they are destroyed by heat and many antiseptics, and then the accompanying fermentation ceases. Their action, according to M. Trècul, is not catalytic, but is a direct result of the nutritive processes of the fungus. It has been objected that vibriones cannot be essential to the origination of putrefaction, because they are to be found in places where putrefaction has not yet begun; but this can hardly be deemed a very important objection, for it is admitted that, in whatever fluid they are found, there actual putrefaction speedily follows; and it is impossible to determine the exact period of the commencement of this process. Putrefaction differs from fermentation in the chemical constitution of the body undergoing change, not in the nature of the process, and doubtless the cause is alike in each. Great names have been arrayed against the fungoid theory, but the weight of opinion is now admitted to be decidedly in its favour; and yet it cannot be said to be proved. For, in a late number of the "Comptes Rendus," M. Bechamp has published an account of some observations he has made on putrefaction in the unopened ostrich's egg, and in which he says that no organised ferment could be discovered.

But, in regard to this latter observation, and indeed to a large amount of negative evidence of this nature, it must fairly be objected that the magnifying power used has not been sufficiently great to enable the observer to assert with confidence that the minute germs of the organisms in question were absent. It must

be remembered that little is known of the early embryonic development of these plants. A reference to plate 58 of Dr. Beale's "How to Work with the Microscope," where bacteria germs are depicted as seen under a power magnifying 3000 diameters, will convince anyone of the fallacy underlying the common remark, "no organised structure was seen," that is, with ordinary object-glasses of a quarter or even one-eighth of an inch focus.

Having premised these general observations, I would now enquire what is the nature and value of the evidence adduced to show the influence of these minute organisms on living structures. And that the argument may be impartial, it will be necessary, first, to consider, although it must be very briefly, the influence of these fungoid forms on the lower animals, and on vegetables.

Common observation proves that vegetables are subject to injury and destruction by the growth of parasitic fungi. The mildew and rust of wheat, probably conditions of the same fungus, Puccinia graminis, are clearly parasitic diseases. A spore, finding a resting-place on the leaf or stem, germinates and sends forth its interlacing filaments of mycelium into the structures of the infected plant, and robs it of its sap.

In like manner Dr. De Bary has shown that the Botrytis infestans is the cause of the potato rot. This fungus is always present in the fully developed disease, and can be inoculated into healthy plants.

Again, the grape disease has, as a constant concomitant, a fungoid growth, the Oidium tuckeri, which is by many believed to be a barren form of Erysiphe. It appears as a white flocculent substance on the leaves, and consists of a densely interlacing mycelium, which, although firmly adherent to the epidermis, does not appear to penetrate the tissue of the leaf; from this base upright filaments rise, which produce sometimes two terminal cells, and sometimes spore-bearing stems, each prolific. The belief that the growth of this fungus is the essence of the disease, is greatly supported by the fact that sulphur, which destroys parasitic vegetable growth, is most successfully used as a preventive.

That in all these diseases, a certain disordered condition of plant is a necessary preliminary for the free growth and wide dissemination of the parasite, is very probable; for nothing grows well except in a soil fitted for it. But that the fungus is the one essential cause of these diseases would seem to be proved by the statements made by those who have paid most attention to the

subject; that the fungus is always present in the disease, and that it can be sown and propagated on healthy plants.

The anatomy of the plant producing ergot is too complicated to be described here. But I may remind you that Tulasne has distinctly proved its fungoid parasitic character; and, moreover, has brought forward much evidence to show that the ergot is the sphacelium or root of a fungus which does not attain its full development until it is planted in the earth, when there arises from it another growth, which is similar to a fungus which causes the death of some caterpillars.

The Torrubia, to which I refer, arises from a spore, which gaining access by the breathing organs of the larval, or even perfect forms of insects, destroys them by the growth of mycelium in the interior, which in time sends up a thick stem having fruit-bearing organs on its summit.

One of the commonest and clearest instances of parasitic fungoid disease in insects is to be seen in the common house fly, which, toward the end of autumn, is constantly to be found stuck in a tetanic attitude to the window pane, surrounded by white mealy particles. These are the spores of the Sporendonema muscæ, which has grown in the body of the fly and killed it. The reality of this fungoid disease is manifest; the mycelium, beautifully white, tubular and ramifying in the body, and sending up on the outside straight filaments supporting sporidia, is proof of the nature and power of the growth. This fungus appears to change its appearance when grown in water, and it would seem to be really only a terrestrial form of the very different Achlya or Saprolegnia.

The evidence that the Muscardine, or silkworm disease, is produced by a fungoid parasite is clear and incontestable; so clear that the microscope is extensively used to detect the infected ova, and by that detection and consequent destruction to effect the stamping out of the malady. The morbific fungus, Botrytis bassiana, consists of a ramifying tubular mycelium and spore-bearing filaments. Its development has been minutely followed by Balbiani, Pasteur, and, very recently, by MM. Bechamp and Trècul.

Time would fail me were I to attempt to describe in detail the growth of these fungi, or to mention even cursorily the many other diseases of vegetables and the lower animals which originate in parasitic vegetation. These instances are sufficient for my present purpose, which is to show that living beings may be injured, or

even destroyed, by the invasion of some of their textures by a parasitic growth of a fungoid nature.

Seeing then that the parasitic theory of disease is true as regards these lower forms of life, it may fairly be argued that it is true, on the face of it, as regards the higher. And so it will not be needful to discuss the general question of the possibility of such an origin of disease, but I may proceed at once to consider whether any human malady is actually caused by parasitic fungi.

More than twenty years ago Remak described certain fungoid filaments which he had seen in favus crusts; since then observations of a similar kind have been multiplied by Schönlein, Gruby, Hughes Bennett, and many others. These filaments have been declared to be vegetable by one set of pathologists, mere degenerations of epidermic substance by another; the one have contended that they were the cause of the disease, the other have denied that they were anything else but an accidental addition; and as time has grown, so has the controversy widened, until now several skin diseases, and half a score of differently named fungi, are the subjects of debate. It does not fall within the scope of this report to discuss the matter in detail, sufficient for me to consider whether any form of skin disease is really caused by parasitic vegetation. Modern observers, among whom our associate, Dr. Tilbury Fox, is prominent, have declared that no true favus exists without the presence of the filaments and spores of a fungus known as Achorion schönleinii; that in the early stages it is seen in the hair follicles, and later in the crusts which have give a name to the disease, and which consist mainly of the elements of the parasite. On the other hand there are not wanting those who combat these views; and some have even contended that these filaments are not vegetable. The latter opinion is absolutely untenable. Dr. Tilbury Fox has well observed that structurally and chemically they differ in no respect from known and admitted fungoid growths; they present the same reaction to iodine, they are unaffected by those agents which dissolve animal tissues and leave vegetable structures untouched, and their tubular mycelium and sporular fructification have no analogous representative in man. M. Remak and Dr. Hughes Bennett have inoculated persons with favus crusts, and reproduced the disease and the fungus. Favus has been propagated by inoculation into rabbits by M. Richter, and in all the cases the fungus was present. He has also been able to cultivate the fungus on apples and cabbages and onions. Favus is

contagious; to show how this might be, M. Bazin made a favus patient rub his head while placed in a current of air, which was condensed on the surface of clean jars, filled with ice, twenty inches distant. In the fluid so obtained spores of the Achorion schönleinii were found. Dr. McCall Anderson has related several very interesting cases of infection of human beings from cats and mice suffering from skin eruption; in both the original and the propagated disease the Achorion was found. M. Gerlach has related a similar case, in which the infecting agent was a fowl. Dr. Salisbury has also made observations of the same nature. Tinea tonsurans is also accompanied by a cryptogamic growth, as well as other skin eruptions. The experiments of M. Ziemsen on the propagation of sycosis seem to me very suggestive. He describes in this disease a free development of fungus spores, and has seen the mycelium penetrating and disintegrating the hair. He inoculated his own chin by rubbing it with diseased hairs. In a week he felt some itching, and then appeared a lump the size of a pea, which was perforated by a hair, and covered by a scab. This had greatly increased in four weeks, and was very painful. The central hair was now drawn out, and after treatment with liquor potassæ was examined under the microscope, when fungus filaments were observed; no growth of this nature was seen in any other part of the scab. In the elaborate and careful account which some recent writers, here and on the continent, have given of these various disorders of the skin, much discrepancy exists as to the number of the parasitic fungi which are observed. I have not space to consider this matter fully, but I may remark that all mycologists now admit, as I have described in an earlier part of this report, that the same plant appears under different forms according to external circumstances; that one form is reproductive, the other merely capable of division; and so I think those who argue for the smaller number of different species have much in their favour. In the face of these facts then, I think it must be admitted that the skin, under certain circumstances, becomes a soil in which fungi will grow and propagate; and that their development is accompanied by certain unnatural conditions, such as scales, pustules, and vesicles, which continue during the life of the fungus, and die at no distant period after its death.

The respiratory tract of mucous membrane has afforded less evidence of parasitic vegetable growth.

Fungi have by some observers been supposed to be the cause of

croup, but the complete absence of anything of that nature in by far the larger number of cases, is proof that they are accidental additions.

In like manner a similar origin has been ascribed to diphtheria. Professor Hallier describes a diphtheritic fungus under the name Diplosporium fuscum; and not only algoid filaments but vibriones and bacteria have been found. More than this, it has been supposed that a connection exists between the prevalence of diphtheria and that of vine disease, and that both depend on the growth of the same fungus. This supposition has not been confirmed. Diphtheria rages when there is no vine disease, and fungoid growths are usually absent from diphtheritic exudations. The influence of vibriones will be discussed further on.

Dr. Laycock has related a case of diphtheritic disease, in which the deposit mainly consisted of Oidium albicans. It spread along the œsophagus, and appears to connect the disease with thrush rather than diphtheria.*

Dr. Hughes Bennett has seen a fungoid growth in the cavities of pulmonary phthisis, and Dr. Crisp has seen filaments of mould on tubercle in the lung of a falcon; but in these cases the growth was clearly adventitious.

The gastro-intestinal mucous membrane has afforded a far larger number of diseases accompanied by cryptogamic growth.

Between the teeth and on the tongue a form of Leptothrix is not uncommon, and vibriones may be found in the foul buccal secretions; these are, doubtless, connected with fermentative and putrefactive changes, but appear to mean nothing more—at least in adults.

In children, thrush, which is so common, is always accompanied by the growth of the Oidium albicans. On microscopic examination the filaments and spores are easily seen; the latter frequently filling the epithelial cells and sprouting there. For long their causative influence was contested, but now it is generally admitted. This fungus is never absent, and it has been propagated on the mucous membranes of healthy persons. It is closely allied to the Torula of yeast, if not identical with it; and is almost certainly, like it, an immature form of a fungus, which in its reproductive stage is called Penicillium. Whenever it is present the secretions of the mouth are always acid; a chemical change which, taken in

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* Letzerich has described and figured fungus cells and spores which he has found in diphtheria.—*Virchow's Archiv., Jan.*, 1869.

connection with the fact that acid fermentation out of the body is caused by the presence of a similar growth, can hardly be looked upon as depending, when, on the surface of the oral membrane, on any but a similar cause.

Dr. W. Smith has described certain concretions of the tonsils, which are caused, he believes, by the growth of a Leptothrix.

Fungi have been found in the vomit, but no organic disease of the stomach has been shown to have a constant connection with any of them. Sarcina has been found in many different disorders. Its square corded bundles can hardly be mistaken. It has been found not only in the secretions of the stomach, but in the fæces, in the lung, in the urine, and even in the crystalline lens, and ventricles of the brain; it has also been seen by Dr. Tilbury Fox on the skin. It lives only in organic solutions, but the story of its development has yet to be told. It can hardly be that these four-sided packs are all its life, and Professor Hallier describes sarciniform cells as the product of the cultivation of ergot. There is reason to believe that it, like the yeast plant, is an agent of some fermentative change, but this is not yet proved; and there is much evidence now extant to show that certain forms of indigestion in exhausted persons depend upon fermentation of the contents of the stomach, produced, as in the manufacture of beer, by the Torula cerevisiæ or some analogous or modified form. M. Schultze has related two cases in which he found in the vomit Torulæ and the products of fermentation; and Dr. Lawson has observed in very many of the cases of pyrosis, the morbid secretion of which he has examined microscopically, the presence of Torulæ, Sarcinæ, and Leptothrix. But much remains to be done in the investigation of this class of diseases. Analogy would point to the possibility of modifications of the digestive processes like the different forms of fermentation, and depending on a similar cause—the growth of fungi; but conclusive proof is still wanting.

Among the numerous theories of the origin of cholera which have been projected, that which attributes it to the reception into the intestinal canal, and growth there, of the spores of microscopic fungi, is at the present moment occupying, in no ordinary degree, the attention of pathologists.

In 1838 M. Boehm published in Berlin a work on Asiatic cholera, and in it he describes the intestinal canal as abounding in cryptogamic growth; sporules apparently developing into filaments like a Torula.

In 1848 Dr. Cowdell argued that cholera depended upon the reception of a fungus by the lungs, which developed in the blood and intestines.

In 1849 Professor Mitchell, of Philadelphia, advocated similar views.

At the end of the same year, Drs. Swayne, Brittan, and Budd, each discovered in the dejections and vomit of cholera patients in Bristol, many round, highly refracting annular bodies; similar, but smaller, bodies they found in the air, and also in the water, of the infected districts; and they called these annular bodies cholera cells or cholera fungi. Drs. Baly and Gull were instructed by the Royal College of Physicians of London to report on this matter, and they arrived at the conclusion that similar bodies to those described at Bristol could not be detected in the air or in the water of infected districts; that many different bodies had been described under the term cholera fungi; that a large number of them were found in substances used as food or medicine; that the origin of the others was doubtful, but that they were clearly not fungi; that they might all be seen in the fæces of others beside cholera patients; and consequently that these bodies were not the cause of cholera.

In 1854 Professor Pacini, in his work on Asiatic cholera, described an innumerable quantity of very small granules or punctiform molecules in the intestinal epithelium and discharges, and believed that these vibriones and their germs were the cause of cholera—the choleraic ferment. Further and more recent observations have been made by Pacini, and he has observed the same growths on the surface of the mucus in white rounded masses. This is the substance described by Klob in 1867 as zoogloœa, consisting of a hyaline matrix containing granules, which develop into beaded filaments.

This fungoid growth was further examined by M. Thomè, who, by cultivation, developed from it a spore-bearing fungus, which he calls Cylindrotœnium. Both observers describe these granules as invading in great numbers the intestinal epithelial cells.

Hallier, in the same year, published his extended observations, in which he describes the fungoid bodies as spore cysts or sporidia, yellow or brownish, consisting of a membrane enclosing bright yellow highly refracting spores. Many of them are round, others irregular in form; they vary in shape, and in the condition of their contents. Often they may be seen breaking up and letting loose the sporules. These multiply by division of their contents, and

develop into very small cells, which become grouped into Micrococcus colonies which float on the surface of the stool. The segmentation of the sporules may begin before they leave the spore-cyst. These bodies, he believes, attach themselves to the epithelial surface of the intestine and destroy it. They often adhere to floating pieces of animal and vegetable food, which then lose their structure, and become much changed. In sugar water branching filaments sprang from them like an Oidium, which occasionally bore sporidia. But the appearance of this further development varied, and was now like a Mucor, now a Cryptococcus, now a Penicillium. When grown in solution of starch the appearances were very similar. On the addition to this of ammonium tartrate a further development was observed, and filaments bearing sporidia, very like the original ones from the stools, were produced. Grown on muscle in sugar water these sporidia were well developed, and the contained sporules grew in the same way as the original ones. In one experiment the Oidium formation was nearly absent, and there was little else formed but spore-cysts containing germinating sporules somewhat like a variety of Urocystis. The experiments were conducted in an apparatus supplied with filtered air, so that no germs could enter from without, and the solutions in which they were cultivated were very various.

I have not space here to enter on the question of relationship between the organisms seen by these different observers, but must hasten to consider shortly, what they are, what their origin, what their relations. Among the uncertainties connected with the account of the development of cryptogams to which I referred in an earlier part of this Report, one thing stood out clear and definite, and that was that all these different forms of fungoid growth, described under so many different names, are not as many distinct and definite species; that one species appears in many different aspects, according to the external conditions under which it grows, such as the soil, the temperature, the atmosphere, &c., &c. The exact sequence of all this has yet to be worked out, and it would be unprofitable, with our present knowledge of this subject, to occupy your attention at any length concerning it. Suffice it to say that at first Hallier contended that this fungus spore which he finds in cholera stools was in its perfection a Urocystis, while Professor De Bary thought it more like a Pleospora. Hallier has now given up his original hypothesis, but he says that he has never seen an Oidium develop into this form, except the Oidium from cholera

stools ; and thus it cannot be originally an European form. It grows only when at a high temperature ; it is then probably of tropical origin, and of tropical forms it is one peculiarly Asiatic. But where is it there to be found? A suggestion originally made by Dr. Tytler in 1833, in a paper read by him before the Medical Society of London, that cholera had its origin in the eating of diseased rice, was seized by Hallier, and he attempted to prove its truth by experiment. He sowed some rice in earth, watered it with the dejections of a cholera patient, and kept it in a warm and moist place. In all cases he found that the germinating grains were penetrated by the mycelium arising from the Micrococcus cells. The young plants grew but feebly and weakly; in three weeks the leaves showed black streaks, and bore perfect spore cysts with sporules. The mycelium penetrated the tissues of the plant, and Cryptococcus cells were seen. On cultivating these spores in different media, he obtained Penicillium, Mucor, and Tilletia forms. Here then is still a weak point. The circle is not yet complete. The spores of cholera stools have, according to Hallier's observations, developed in the young rice plant, normally, to their perfection, but the fungi so bred have not yet produced cholera. And the great authority on all questions relating to fungi, the Rev. J. Berkeley, in his address to the British Association at Norwich, says that there is no evidence to connect this Urocystis, or whatever it may be, with the rice plant. He asserts that there is no proof of it being a common disease in India, or in other tropical regions, for in Dr. Curtis's collection from the United States, amounting to 7,000 specimens, there is not one of rice with any endophytic fungus ; the only cryptogamic plant found after close search being a superficial fungus like ordinary mould. This appears to me, although a strong objection, yet not a fatal one, for Hallier believes his cholera fungus to originate in India. The mode of propagation and the experience of later years as to the preventive treatment of a cholera epidemic is not inconsistent with this fungoid theory of its origin; its frequent propagation by water contaminated with the evacuations of cholera cases, and the great success which has attended a methodical disinfection of the excreta from cholera patients by chemical agents, which would destroy all organic germs present in them, harmonise well with this hypothesis. As yet no Micrococci have been found in the blood ; but Dr. Parkes thinks he has seen them in the urine first passed after the algide stage. He also says that in 1849, and in later epidemics, he saw the spores which Thomè and Hallier

have figured. But much is still wanting to make the theory a fact; much better knowledge of the development and growth of cryptogams generally, much more accurate and complete information as to this special cryptogam. Until this knowledge is obtained, speculation as to its mode of action is futile.

In the secretions of the genito-urinary apparatus fungi have been found. Dr. Beale has related a case in which fungi resembling spermatozoa existed in the urine of a person sinking from joint disease. It contained albumen, numberless vibriones, and these fungi, some with, others without heads. After death headless filaments were found in the pelvis of the kidney, but no growth of any kind in the tubes.

Dr. Tonge has reported a case of phthisis in which, on post-mortem examination, a fungoid growth was found in the kidney. The pelvis contained the sporules and mycelium of a species of Oidium, the filaments of which penetrated into the medullary cones. A year previously the patient had diabetic symptoms, but there was no sugar in the urine a little while before death. In this case the existence of the fungus during life is not certain, although probable.

Dr. Salisbury, in a late number of the "American Journal of Medicine," describes forms of fungi under different names, which he has seen growing in the epithelial scales and the secretions of the genito-urinary apparatus. He says they create considerable irritation, catarrhal discharge, and such indurations as may be mistaken for scirrhus; they attack the epidermic cells around the urinary orifice, and cause troublesome itching; they grow in the epithelium of the glans penis, producing minute papulæ and excoriations; and they may spread from it to the urethra and set up a gonorrhœal discharge. One form follows malarial fevers, one produces enlargement of the womb, and another simulates stone. And he not only credits fungi with the production of gonorrhœa, but he sees in them also the cause of syphilis. In more than one hundred cases, he says, he has cut out the base of the chancre, and then he always finds vegetable filaments running singly and in bundles through the diseased connective tissue; and in the secretion of the sore he meets with a small highly refracting sporoid body. More than this, he has met with the fungus in the blood, growing and fructifying. Dr. Salisbury is a great believer in the potency and the variety of these fungoid growths, and he finds them almost everywhere. Professor Hallier asserts that he

has succeeded in showing that the micrococcus of a Coniothecium is present in gonorrhœa, chancre, and secondary syphilis; but there is no evidence of its being the cause of the disease. A similar and perhaps identical Micrococcus, he says, is to be found in the blood of glandered horses.

A very interesting account by M. Ordonez, in the "Comptes Rendus," of fourteen cases of supposed glandular tumours, appears to me of great importance. It opens up a new subject, which deserves further investigation. He describes a mycelium with branching vesiculated filaments arising from it, the enlargements resembling sporangia, and containing bright corpuscles like spores. The filaments and corpuscles presented the usual reaction of cellulose.

This is more especially interesting when taken in connection with the investigations of Dr. Carter and Rev. J. Berkeley on Mycetoma or the fungus foot of India. This disease attacks the hands and feet, which become much swollen in a more or less globular form, dark coloured, and in the end perforated with sinuses. Fetid sanious pus is discharged along with nodules of vegetable matter of a firmish consistence. On examination, the bones of the foot and the pedal ends of the leg bones are found to be riddled with cavities of different sizes, filled with a reddish mass like a small truffle, which presents under the microscope short beaded tawny thread-like filaments arising from a common base, and often bearing on their tips large spore-like cells. These enlargements become enormously dilated, assume a resinous consistence, and burst, but do not appear to germinate. The nodules seen in the discharge are parts of the original mass which have separated. When the truffle-like substance is cultivated after death a red mould springs from it; but as yet its source has not been traced. The fungus in the foot is clearly an imperfect condition, but it is no less clearly the absolute cause of the disease.

For long it has been believed that intermittent fevers were caused by the vapours arising from low, moist places or marshes. Sometimes the poisonous agent has been believed to be microscopic animalcula, sometimes a flocculent albuminous vegetable matter. The condensed air of malarious districts has been examined, and found to contain vegetable cells and fibres, grains of pollen, debris of insects, infusorial animalcula, and unicellular fungoid plants. Since 1862 Dr. Salisbury has turned his attention to this matter. First he examined the morning saliva and expectoration of ague

patients, and found, among other variable organic and organised bodies, one constant presence ; minute oblong cells, single or aggregated, with a distinct nucleus, a smooth cell wall, and a clear intervening space. These bodies were only found in the secretions of persons living in the malarial levels, never in those living in the non-malarial higher lands. To discover their origin he suspended large glass plates a foot above the surface of marshy places during the night. The condensed fluid on the under surface of the plate contained many of the organisms met with in the expectoration, but none of the special small cells ; these were, however, found on the upper surface of the plate in considerable numbers. He repeated this experiment in many different places, and at many different times, and always with the same result. Passing over a half-dry peaty bog he invariably experienced an uncomfortable sensation in his fauces, and on examination of his expectoration afterwards he found it filled with the minute oblong cells above described. Over this bog he suspended his plates for the night, and found now the under surface studded with the cells. Parts of the bog were covered with a white incrustation, which proved to be aggregated masses of the small cells met with in the expectoration of ague patients, and were evidently the product of Palmelloid plants. Further experiments showed him that these bodies rise in the air during the night, and fall after sunrise. He relates a number of instances in which he traced the occurrence of ague to the recent growth of this palmelloid, which he calls Gemiasma ; and asserts that among his numerous observations he has never met with a case of ague *in situ*, where he failed to find those plants growing near, and that whenever he found them growing in an inhabited locality, there intermittent or remittent fever, or both, prevailed in proportion to the extent and amount of growth of the fungi. In the urine these little plants are to be found growing just as they do in the bog ; they appear as small cottony flocks scarcely noticeable by the naked eye, and too few to produce turbidity. They are numerous in proportion to the long continuance and severity of the disease. In severe cases the urine rapidly undergoes the acetous fermentation ; so rapidly that in a few hours after voiding, fertile filaments of Penicillium, Aspergillus, &c., may be seen. He does not look upon these forms as essential to the disease, but as indicating the presence of glucogenic matter. He then proceeded to the production of intermittent fever by experiment. He selected a hilly district, five miles from any malarious locality, where ague had never been

known, filled boxes with earth from a malarious prairie bog on which the Palmella was growing freely, and placed them on the window sill of a bedroom on the second floor in which two young men slept; both got tertian ague within a fortnight, whilst four other people who slept in the same house on the ground floor were not affected. He relates two other experiments of the same nature, which had a similar result. These observations are very remarkable, but they are by no means conclusive. To the synthetical experiments it has been objected that the cause of the ague might really be in the box, and yet not necessarily be the Palmella; for there was much peaty soil also, from which the poisonous emanations might have proceeded. Then, the spores have not been seen in the blood, where, if morbific, they should, in the absence of any local lesion, be found. Again, if the spores are so plentiful in the air, it is hard to see how they could fail to be found in the saliva, and in the chamber utensil. At the present time it cannot be said that the fungous origin of intermittent fever is proved, but it must be admitted that experiment alone can demonstrate the inconclusiveness of Dr. Salisbury's observations. More than twenty-five years ago Professor Morsen, of Liège, taught a similar doctrine. He believed that Algæ, at the period of fructification, were instrumental in the production of intermittent fever, and that the spores were the immediate cause.

Dr. Salisbury has endeavoured to demonstrate that measles are caused by the spores of fungi, which are developed on apparently sound straw when packed up close and kept in a damp atmosphere at a temperature of 19° C. or upwards. His attention was first drawn to this subject in 1861 by a case of severe feverish attack, with an eruption like that of measles, apparently caused by the dust arising from mouldy straw. It was also found during the Rebellion in America that soldiers on first going into camp were seized with a disease closely resembling measles. These soldiers all slept on straw beds. The spores of the mould when inoculated into the arm produced a feverish attack with catarrhal symptoms and weakness of the eyes, but only trifling red blotches on the skin. In a school of 175 boys, where measles had broken out, he inoculated twenty-seven with the spores; only one had the disease afterwards, but it continued to spread among those not inoculated. I need not point out the unreliability of statistics such as these; and in regard to the measles among the soldiers, Dr. Wood of Pennsylvania, asserts that camp measles could very frequently be traced to ordinary

sources of infection. That a feverish attack with a skin rash may occasionally be produced by the inhalation of fungi spores is more than probable, but this is not measles. Dr. Kennedy, of Dublin, relates the case of a boy in perfect health who had a quantity of mouldy flax seed thrown in his face, which at once produced smarting and watering of the eyes, running from the nose, cough, and dyspnœa. The eyelids were red and swollen, and there was much fever; he looked like a case of measles without the rash. I venture to assert that there is not a shadow of proof that such as these are cases of measles. Other substances besides the germs of mould produce the same phenomena. I know a young man who has catarrhal symptoms of this kind whenever he handles linseed meal. Now, as this always happens, it cannot be supposed to arise from the intermixture of fungus spores, but rather from the minute particles of the meal itself. Professor Hallier has examined the expectoration and blood in measles. In the former he found a delicate Micrococcus in great quantity, and also spores or spore-like fungus cells, both round and oval. Similar small Micrococcus cells were found in the fresh blood. When cultivated he obtained forms of Mucor mucedo. He notes the fact that the micrococcus of the same fungus is found in the lungs of cattle dead of epidemic pleuropneumonia, in connection with a suggestion which has been made previously that this disease has the same cause as measles.

The following relation shows that the spores of some fungi do produce disease. The Arundo donax is much cultivated in France for industrial purposes. After the stems are cut, they are gathered into heaps and exposed to the air. In the course of time they often become mouldy, and the workers, adults and children, are then frequently attacked by a strange, severe, and sometimes fatal malady. Dogs have been known to be similarly attacked after sleeping on the mouldy heaps. The disorder is described as follows:—fever, heaviness of head, vertigo, and swelling about the eyes and mouth, extending to the head, which often becomes very large. Then vesicles and pustules appear on the skin, often attacking the respiratory and alimentary mucous membrane, producing dyspnœa in the one case, and vomiting and diarrhœa in the other. At times there is great swelling of the genital organs with much sexual excitement. This disorder is now admitted to be produced by the spores of Ustilago hypodytes, a form of smut, which begins by a grumous whitish mass of reticulated mycelium, growing at the expense of the infected plant, and finally becoming converted

into a mass of round and often fetid spores. Although the evidence in favour of the malignant influence of these spores is strong, it does not follow that all fungus spores are equally morbific, indeed it is known that they are not. Leplat and Jaillard, for instance, have injected spores of Oidium tuckeri and Penicillium glaucum into the veins of dogs without producing any evil result.

It is now necessary to take up the other part of the subject, and indeed the more important part, viz., the influence of Vibriones, Bacteria, and Bacteridia; remembering always that all recent investigations are tending to show that these structures are very probably in most instances germinal forms of higher fungi or algæ.

It will be advantageous to consider first, the anthracic and anthracoid diseases of cattle; these are various in their phenomena, but apparently one in their essence; and, although described under many different names, splenic apoplexy, braxy, sang de rate, hog cholera, parturition fevers, black quarter, &c., may well, for the special purpose of this Report, be considered in bulk. They are all blood diseases having different local manifestations. Generally sudden in onset, they are rapidly fatal, and the dead body soon undergoes putrefaction. Splenic apoplexy is not only the most common of these diseases, but it is the one which has been the most carefully studied. The post-mortem appearances of these disorders are essentially alike. The blood is dark, generally fluid, and coagulates afterwards very imperfectly. The veins are gorged, and there are frequently extravasations. Putrefaction often commences before death. In splenic apoplexy there is great transudation of blood into the spleen, which attains a very large size. Symptoms during life, evidences after death, alike point to the blood as being or containing the essence of the disease. M. Davaine asserts that it is always crowded with those minute filaments which, from their want of mobility, he calls Bacteridia, in contradistinction to the actively moving Bacteria. They are often simple, sometimes imperfectly segmented. He has inoculated healthy animals with the blood of animals dead or dying of anthracic diseases, and they have died of the same disorder; in all cases the blood was full of Bacteridia. Certain animals, such as the dog and birds, are insusceptible of this disease, and in such cases, although the inoculated blood was full of Bacteridia, they ceased to live and to be propagated in the blood of the injected creature. He also asserts that the growth of Bacteridia is always the first morbid change, and that they precede the general symptoms. He has inoculated animals

and watching closely at short intervals the condition of the blood, has seen these structures many times whilst the animal appeared quite well; but they always increased rapidly with the progress of the disorder.

There seems little reason to doubt that man also may become affected with a blood disease, having a special local manifestation akin to these anthracic diseases of brutes, and that malignant pustule at least is one of perhaps several forms. It is doubtful whether this disease can be inoculated from man to man, as proof is still wanting; but numerous cases render it absolutely certain that it is a frequent result of contamination with the anthracic diseases of cattle. The researches of Davaine and others have shown that the pustules are little else than masses of Bacteridia. Removed on the second or third day of their development, hardened with chromic acid, and then treated with liquor potassæ to dissolve the cutaneous structures, Bacteridia may be seen in myriads. At first confined to the inoculated spot, they soon propagate themselves in the blood, and then grave constitutional symptoms ensue. The blood is after death found to be semi-putrid, uncoagulable, and effused into the structures of the internal organs. It is black and treacly, and proceeds to complete putrefaction with great rapidity. The blood discs are irregular and agglomerated in small masses, in the clear spaces between which, may be seen Bacteridia like those found in animals dead of sang de rate. Rabbits fed with the pustule took splenic apoplexy, and this again communicated malignant pustule. The blood, containing Bacteridia, of a man dead of malignant pustule, has been injected into a guinea pig; in two days the animal died, and its blood contained numerous Bacteridia.

Now what is the value of this testimony? So far I think it must be admitted that the evidence is only circumstantial. This thing seems certain, that inoculation of the blood or purulent discharges of anthracic diseases, whether of men or brutes, will reproduce a similar disease; and that this blood contains those minute organisms called Bacteridia, in great numbers.* But the blood is semi-

* Whilst correcting this for the press I notice in the "Comptes Rendus" of January 11, 1869, that M. Bouley has communicated to the Academy the results which have been arrived at by a Commission, of which he was President, appointed to report on the *mal des montagnes*, a malady which appears to be similar to, if not identical with, malignant pustule. He is of opinion, from this inquiry, that the blood of an anthracous animal can transmit an anthracic disease, even when no Bacteria can be seen by the

putrid, has undergone great chemical change. What then is the virus? The vegetable parasite, or an accompanying, if not resultant, putrefaction compound? To make an approach towards an answer to this question other considerations must be reviewed. MM. Leplat and Jaillard have recorded a series of eight experiments in which they injected into animals putrid vegetable infusions containing Bacteria, without producing any evil effects; but a ninth in which putrid blood was used was fatal. From this they conclude that it is the putridity of the blood, and not the accompanying organized growth, which is morbific. On the other hand, M. Davaine has shown that as putrefaction progresses the Bacteridia found in these anthracic diseases are killed; and he asserts that the blood in which the Bacteridia are living will propagate the disease, but that as soon as they are killed the inoculation is of no avail in developing the special disorder. And in connection with this matter he says that the Bacteridia increase in number in proportion to the intensity and rapidity of the disease, and to the nearness of death. Again MM. Coze and Feltz have published a very extended series of observations on infection by the blood, which they divide into putrid, typhoid, and variolous. They say that when putrid liquids are injected into the blood of rabbits death takes place in thirty or forty hours, after great elevation of temperature. The blood is greatly changed, it contains less oxygen and more carbonic acid; and Bacteria of considerable size are found in it. They appear to act the part of ferments, a change for which blood, a warm, alkaline fluid containing fermentable matters, is not unfitted. Although the fermentation of the blood is not complete, the preliminary stage of putrefaction has apparently commenced, and is rapidly completed after death. It seems as if the initiative of this change by the Bacteria were so sudden and wide spread in some instances that death takes place at once.

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microscope; that anthracous blood which contains large numbers of Bacteria, loses its virulent property by drying, and does not recover it on the addition of water, although the Bacteria still continue perfectly visible; that the blood of rabbits killed by the inoculation of anthracous blood always contains Bacteria, although the inoculated fluid did not contain them; whilst in ruminants, dead under the same conditions or of the disease taken naturally, Bacteria are not always found, and, when they are not present, the blood is still equally active. This difference, I suspect, will be found to depend on imperfection of observation or of microscope rather than in want of uniformity in the morbid changes occurring in the same disease in similar animals. The details of the experiments are not yet given.

Blood taken from a living typhoid fever patient, and injected into rabbits, produces notable results, fever, and high temperature; and the blood of the infected rabbits will reproduce the same disease in other rabbits. Bacteria are always found. Successive generations of the disease are more virulent, and in proportion to the virulence so is the activity and number of the Bacteria. Affection of Peyer's patches, although it occurs, is far from an universal pathological condition. The blood contains less oxygen and more carbonic acid. The injection of variolous blood induces very similar phenomena. Again the power of the infectious element increases with successive inoculations from rabbit to rabbit. Small-pox blood is very fatal, it kills in ten hours, and the requisite dose is very small. There is great elevation of temperature, 44° C. Bacteria are found in enormous numbers. Thus then in these experiments we are again shown the close relationship between Bacteridia and infectious blood. Where from the injection of putrid, typhoid, or variolous blood into rabbits, fever and death is caused, there Bacteria are found in great numbers; but yet this is not conclusive; they may after all be merely the accompaniments, constant though they seem to be, of that chemical change of the blood in which alone resides the essence of the disease.

To elucidate this matter Davaine relates the following experiment:—A guinea pig well advanced in pregnancy was inoculated with the blood of another guinea pig which itself had been inoculated with the blood of a man who had died of malignant pustule. It died two days afterwards. There was only one fœtus. Myriads of Bacteridia were found in the blood of the mother and in the placenta, but none in that of the fœtus. Immediately after the examination one guinea pig was inoculated with the placental blood; it died as its predecessor, and the blood was full of Bacteridia. Three others were inoculated with the blood of the fœtus, but they had no symptoms, and were well many months afterwards. The argument here is, that the poisonous principle could not be dissolved in the blood, for if so it would have diffused into the fœtal blood, and so this would have been, as it was shown not to be, as deadly as that of the mother. The poison then must have been solid, and of such a size as to be arrested by the capillary walls. The blood contained a solid not existing in it during health, and that solid was the organised entity known as Bacterium or Bacteridium. This seems very conclusive. I say seems, for an isolated experiment of this kind cannot really be accepted as conclusive in a matter of such

obscurity. And although it fixes much suspicion on the Bacteridium as the noxious element, an objector might say, I grant you the first half of your proposition, but the second is not proved; you admit that the blood corpuscles are very much changed, where is your proof that death does not start here? And to this question it must be answered, that the proof is yet wanting; although the further observations of M. Davaine are not without pertinence and weight. He asserts that the complete putrefaction of the blood destroys the Bacteridia of anthracic diseases, and that the injection of this putrid blood, whilst often followed by the gravest results, does not produce the same symptoms as the injection of blood in which the bacteridia are still living. If this be correct, there is some basis for the doctrine of the specificity of these organisms, and indeed there are some who contend that there are forms of Bacterium and the allied species, peculiar to certain diseases; that malignant pustule, small pox, typhoid fever, &c., have each their own special and peculiar Bacterium, capable of producing its own, and only its own disease. It will be well to examine the grounds of this belief.

I have already in an earlier part of this Report shown the very imperfect condition of our knowledge of these structures, and have given reasons for believing that many of them, at least, are immature forms of some higher plant, and it may be, in the case of a few, of some animal. The organisms themselves, then, afford, in the majority of instances, no generic differences of structure or appearance. They are found in the intestines in diarrhœa, dysentery, and cholera. On all hands it is admitted that Bacteria are present whenever ordinary putrefaction is progressing, and consequently it would be strange if they were not found in this class of disease; and not only in this, but generally in fæcal matters. Indeed they are so found; and in certain birds, as ducks, pheasants, common fowls, they exist in great numbers in the intestines. The only experiment bearing on this matter with which I am acquainted is one related by Dr. J. G. Richardson, who after drinking putrid water containing large numbers of Bacteria, found them in considerable quantity in his blood; and at the same time he had headache, furred tongue, dry throat, and a slight diarrhœa. Dr. Davaine remarks that the Bacteria of diarrhœa lose their movements as the stools grow cold, and that consequently they differ from those which appear in two or three days as the evacuations putrefy, although to the eye they are alike. In putrid urine they

are seen as in all putrefying things. I have seen them in the phosphatic urine of mentally exhausted men at the time of its passing, and again I have seen granular phosphates mistaken for them.

M. Poulet has accounted for the contagion of whooping cough by the dispersion of Bacteria in the air, which are present he says in large quantities in the expectoration in this disease. On the other hand, M. Bouchut has examined the secretions of the respiratory tubes, immediately after expulsion, without finding any Bacteria, but they appeared a short time after when putrefaction was commencing.

Influenza has been held by many to be caused by microscopic growths, and it has been supposed to be connected with epidemics of fungoid parasites in vegetables; but there are no reliable observations on this point. Neither has there been, as far as I am aware, any attention paid to the presence of Bacteria in any microscopic examination of the blood in this disease. Bacteria have, indeed, been found in the blood of a horse dying of this disorder, but there are no facts to show that this is a similar disease to the epidemic influenza of man.

In purulent secretions of all kinds Bacteria have been seen, whether the secretion be from respiratory or nasal or any other mucous membrane, from a simple ulcer, or a chancre, or a cancerous sore; but they are only found when putrefaction is proceeding.

We have already discussed the observations of MM. Coze and Feltz on the blood of small pox. These gentlemen have seen Bacteria in the fluid of a pustule in the case of a young man who had small pox and recovered. In a child dead of this disease they have seen the same structures in the blood and in the pustules, but they have found most in the spleen. In their experiments on rabbits we saw that the injection of variolous blood was rapidly fatal, but there was no eruption. The blood was poisonous, but it was not proved that it was in its sole capacity of small-pox blood, containing the Bacteria of small pox.

The case of the injection of typhoid fever blood is stronger, but far from conclusive. Peyer's patches were sometimes affected, but not always, and again here we must wait for further observations.

Lastly, there are the experiments on anthracic diseases and their congeners, and if we grant the close relationship between these disorders and malignant pustule—and this can hardly be denied—there is much apparent ground for the assertion that the inoculation of this anthracic blood will produce an anthracic disease and none

other. Both M. Davaine and MM. Coze and Feltz assert that simply putrid blood does not produce the same results as the blood of these specific diseases or of small pox. A careful consideration of the statements as yet laid before the profession must, I think, lead to the conclusion, that while there is much yet to be done before satisfactory proof can be given, the theory of a special anthracic poison is by no means shown to be untenable. This becomes even more probable if we take an enlarged view of these disorders. For myself I believe that their essence is rapid putrefaction of blood, that they, and puerperal fever, dissection wounds, some forms of septicæmia, hospital gangrene, and perhaps other similar diseases, are at bottom one and the same. True, the local manifestation varies, but the general symptoms are alike in all. Indeed these very Bacteria have been found by Mayrhofer in the discharges of puerperal fever patients, and by many in dissection wounds. The whole subject is only now emerging from the obscurity in which the dreams and fancies of a former time, and the imperfect observations and crude theories of a later, have enveloped it. There is, as the conflicting experiments and opinions which I have related show, much to be learnt, much to be reconciled, but there is nothing essentially untenable in the idea that Bacteria cause putrefaction, that differences in the chemical products of putrefaction occur in accordance with differences in the organised ferments, and so that different classes of symptoms result. I think that the latest experiments point to some such solution of the question, and not to that which supposes a different Bacterium for each different disease. For I think it must be admitted that the effect of the Bacterium-bearing blood is only made cognizable to the senses by the symptoms resulting from the action on the living structures of the possibly Bacterium-produced chemical compounds which it contains. Such a change in the blood may be produced in the course of any disease, and would seem, I contend, rather to be the cause of the special mode of death than of the special form of disease. Again, it must not be forgotten that having regard to the identity in essence of fermentation and putrefaction, that the latter process may include, as does the former, several varieties, differing from each other, either by a different organised ferment, or by a difference in the stage of its growth ; and, on the other hand, it may be that these differences in the appearance of the ferment may entirely depend on the nature of the medium in which they are growing.

Looking back over the whole subject it is impossible to deny its importance and its wide range. Some things stand out clear and true, but much is obscure. Gathering up my thoughts and opinions they seem to lead me to the following conclusions :—

That the organisms described as Bacteria, Vibriones, and Bacteridia, are, equally with the higher forms, as Achorion, Ustilago, &c., vegetable growths, and are probably embryonic conditions.

That the same plant takes many forms, depending on its stage of growth, whether it be young or old, and depending on external circumstances, such as food, light, and heat.

That these plants propagate in all their forms by division of the individual, and in their adult state by reproduction of their kind.

That there is not at present sufficient evidence to prove that they are propagated in any other way than from germs derived from a parent of like kind; and consequently that the theory of spontaneous generation cannot be accepted as a demonstrated fact.

That the germs from which they arise are very minute, can be dried for years, can be frozen, and can be heated above the boiling point of water, and yet retain their faculty of growth when placed in proper conditions.

That fermentation is produced by the growth of a plant in the substance undergoing the change ; and that it is highly probable that the different varieties of fermentation, vinous, acetous, lactous, &c., are produced either by different plants, or by different stages of maturity of the same plant.

That as putrefaction differs from fermentation only in the character of the original fluid and the resulting products, not in the nature of the chemical change, it is not likely that its essential cause is of a different kind.

That local changes producing death of the part, and general changes producing death of the whole body, are produced by parasitic vegetables in plants and in the lower animals.

That such parasitic vegetables are not developed from any part of the infected organism, but grow from germs originally introduced from without.

That similar parasitic vegetables produce special diseases of the skin and of the bones in the higher animals and in man.

That the growth of parasitic vegetables in the stomach produces unnatural fermentative changes with definite morbid symptoms.

That a parasitic vegetable growth on the tongue is the cause of thrush and like diseases.

That the evidence adduced in favour of the dependence of ague and cholera on parasitic vegetable growth, is still insufficient for the proof.

That the evidence adduced in favour of the dependence of syphilis and gonorrhœa on parasitic vegetable growth is very weak.

That there is sufficient evidence to prove that diphtheria, croup, measles, and whooping cough, do not depend on the growth of parasitic plants.

That putrefactive changes may be produced in the blood of a living animal when blood containing Bacteria is injected into it.

That analogy and experiment both point to the Bacteria, as the cause of this putrefactive change.

That it is probable that varieties of putrefactive change may be produced, either by different Bacteria or by different stages of growth of the same plant, and hence that different classes of symptoms may arise.

That such putrefactive change of the blood may occur in many different forms of disease, which, although differing in name and in primary local manifestation, are the same in nature.

That the morbid symptoms are the direct result of the foreign chemical compounds, probably produced in the blood by the agency of the Bacteria.

That putrefactive change of the blood, so induced, may occur in the progress of any disease, and produce distinctive symptoms.

That there is not sufficient evidence to show that specifically differing Bacteria produce specifically differing diseases.

Such, Mr. President, seems to me a fair and reasonable view of the subject which I have ventured to bring before you and my Brother Graduates, for myself, asking you of your kindness to credit me with an honest, albeit feeble, attempt to gather into a focus the scattered rays which throw some light on a vast and difficult problem, but, for the work itself, asking only that it may be judged with a true and righteous judgment.

CLINICAL NOTES.

By George Cordwent, M.D., F.R.C.S.

DELIVERY WHILST STANDING; IMMEDIATE DEATH.

Mrs. S——, aged 26, of good formation and health, advanced without an unfavourable symptom to the full period of pregnancy. She became in labour about 3 a.m. At her strong desire she was permitted by the nurse in attendance to remain standing. At 8 a.m., whilst the attention of this inexperienced nurse was misdirected, the infant was expelled, and, falling to the floor, dragged with it the placenta. During about a minute subsequent the patient continued to support herself by a pillar of the bedstead. She then said, "I feel faint and giddy, lie me on the bed." Her request was obeyed, and she almost immediately expired. I saw the deceased a few minutes after death. The countenance was placid, and the whole pose consistent with the statement of the attendants, that there was no contortion nor apparent pain between the expulsion of her child and her death. I inspected the womb, abdomen, and chest, thirty hours after. The womb, both externally and internally, presented the appearance ordinary immediately after childbirth, except that there was probably less blood in its cavity than usual; there were no clots; the placental surface was not lacerated; the abdominal organs were healthy and normally arranged. My attention was immediately arrested by a great distention of the coronary vein of the stomach; it was quite tense by air; it contained no blood, or only the finest shred of this along its attached border. I did not find air in any other vein. The viscera of the chest were quite healthy. I cannot speak positively to there being air bubbles in the right cavities of the heart. My impression is there were; but other duties pressing at the time

prevented the minute and patient examination this remarkable case so highly deserved. The brain was not examined.

That this healthy young woman died from air absorbed into the uterine veins, suddenly opened by the placenta being torn from its attachment, cannot reasonably be doubted; and the case pronounces strongly at least one *immediate* danger attending hasty removal of the placenta; and though I do not know a similar recorded case, I cannot doubt some have occurred. Then again, though all the conditions happily will seldom exist, by which atmospheric air may pass into the veins, still, as air often, perhaps always, passes freely into the cavity of the uterus after hasty and exhaustive delivery, of course it bears with it any contained absorbable poison. Let us think of the action of this on the unprepared placental surface of the uterus, raw by sudden exposure. Besides such probably frequent cause of evil, a meddlesome removal of the placenta is a dangerous insult to nature's competence in her first principle. We see by the mechanism of child-expulsion, nature relieving the womb by stages; and why, after this exhaustive effort and collapse of tumour, should we rule to remove the placenta in twenty or thirty minutes. If the infant be healthy, the placenta can have no extensive diseased attachments; and surely, under such circumstances, the womb that can expel its child can expel its placenta. Why then interfere? Instead of allowing it to remain *in utero* half an hour, why not allow it to remain five or six hours, if not naturally expelled? I *believe* uterine disorders are often consequent on impatient removal of the placenta, and *I know* that hour-glass contraction is; besides, if it be true, that sudden removal of the support of accustomed pressure is hurtful, and sometimes immediately dangerous, this is another reason for delay, and more cogent in proportion as the child has been hastily expelled. Of above 2,000 childbirths which have come under my care or observation, I do not remember one in which the child being properly developed, and at full period, there was reason to think the placenta would not have been born without interference, and under such ordinary circumstances have never known an evil arising from delay, but I have known many reasonably attributable to that hasty removal, so temptacious after protracted labour; after, in fact, what makes calm delay especially needed.

PERSISTENCE OF SYPHILITIC TAINT.

Mr. H——, a vigorous man, of fine frame and bilio-sanguineous temperament, aged 23, contracted syphilis in April, 1857. In due course he had bubo, the scaly eruption, sore throat, &c., in fact, constitutional syphilis. He became somewhat thinner, and complained of debility, but did not discontinue his active business duties, except during about a fortnight, whilst suffering a pustular eruption on the foot. He was treated in the ordinary way, at first by mercury, then by iodide of potash. After seventeen or eighteen months there was no symptom of syphilitic poison or disturbance ; and continuing in all respects apparently well, he married on the 1st of February, 1859, a healthy young woman, aged 20. Her health had been good from childhood; on April 16th, she aborted at about the second month. In the latter part of September or beginning of October, she aborted, after a little more advancement. On October 7th, 1860, she aborted at the end of the fifth month. There were two abortions in the interval for which I did not attend. On the 29th of March, 1864, she had a female child, at full period ; it was born alive, but died almost immediately after; the labour was quick and natural. Two miscarriages were reported to me as having occurred between this birth and that of October, 1860. The hair and nails were said to be formed on each child. On April 9th, 1865, a male child was born, again at full period; it was well formed, and not otherwise remarkable than that the skin was paler than usual. It sucked, and appeared to do well for a fortnight. Then an eruption, unmistakably syphilitic, appeared, first on the forehead, face, trunk, and limbs ; the nostrils and fauces were also severely affected. The eruption was preceded by oppressed breathing. The child died at the end of eleven weeks. There was no post mortem examination. In 1866, Mrs. H—— was again confined of a well-formed female child, still living. I did not attend the birth, nor watch the early progress of this child, but have very lately seen her. She is well-grown, active, and without indicative peculiarity. Her weight is, perhaps, less than that of most children of her age, and the front teeth are decayed. The mother reports that *five weeks* after birth, this child was attacked with a *pustular* eruption, which spread from its first seat on the forehead, to the scalp, and that the incrustations were brown; the nostrils and fauces were also

affected; no cicatrices are observable. The eruption continued, more or less, during three or four months, but since then the child has had fair health.

Mrs. H—— is now far advanced in what, I believe, is her twelfth pregnancy. The husband has continued active, and in apparently excellent health since his supposed recovery from syphilis in 1858.

We see, I think, in this case, with what latent subtlety the poison of syphilis may lie in the system of the father, destroying or tainting the offspring, as time or constitution may or not assuage its deadly vigour; and this, apparently, without affecting the mother, in womb or constitution, except to the extent the former may suffer by contact with a dead child. We see clearly too, that whilst this subtle virus may elude approved treatment, it was in the instance before us, at least, mitigated by slowly reducing vital processes. But when we see the inherence of the poison and its early behaviour, can we doubt that when life to the germ is at length spared, taint is continued in forms varying with the modifying conditions of the afflicted. And in the absence yet of quite definite proof, instinctive reason unavoidably suspects evil largely to alloy the attributed good of vaccination. For as lymph from a typical vesicle of a vaccinated person tainted with syphilis, may not be distinguishable under even the highest magnifying power, from that of an untainted person, so neither, probably, could the spermatic fluid, capable of life, be distinguished from that whose effect still proves deadly. But if not the lymph, the almost inseparable ichor may do the work of contamination; and in the business energy lately promoted by the state, public vaccinators are not likely to be deterred by minute inquiry into antecedents.

PERFORATING ABDOMINAL WOUND.

On August 1st, 1867, F—— D——, a fine healthy boy, aged 14, was gored by a bullock, and a laceration, of about three and a half inches long, made in the abdominal wall, a little to the right of the umbilicus; many feet of small intestine immediately protruded. He was placed in bed, and carefully covered; remaining in this state about two hours, till my arrival. The bowel was uninjured, and was returned without difficulty; the sides of the wound were

brought together in the usual way by pins, and other support carefully applied. The young patient appeared much alarmed, but there was no evidence of diseased action, in fact, evidence of none. No medicine was given. The injury was done within half an hour after he had taken dinner of animal food and two kinds of vegetable. At 2 o'clock next morning he vomited twice, ejecting the dinner, of which all the several articles were not only distinguishable, but as little altered as could be after mastication. During the seventeen hours' interval between my first and second visit, he had suffered no pain beyond momentary slight gripings. He had taken a little milk and water, but beyond, at first, a desire to moisten the mouth, he had no thirst during the first four days after the injury, and there was also a complete absence of hunger; there *was no nausea;* and in this long and remarkable absence of hunger *and thirst*, there appeared *no aversion* to food nor fluid, but simply no desire. The bowels moved without medicine on the fourth day, and in the latter part of this day ordinary appetite returned. The pins were removed on the fifth day. Sleep during the first night was broken, but afterwards it was quite natural; and throughout the case there was no bad symptom, no evidence of constitutional disturbance. The curious and highly interesting feature is, that with all this absence of fever and pain, and with sleep natural, there should still be in this growing lad, no hunger, when digestive *action* would be likely to fatally affect the traumatic lesion. That digestion ceased from the time of the wound, is evident by the particulars of the subsequent vomit, which was followed by no nausea. How much this case shows the reciprocal sensibilities of animal life: the affinities of digestion in a growing youth immediately ceased on a traumatic lesion, which scarce otherwise disturbed the general health.

SOME PRACTICAL OBSERVATIONS ON FISTULA IN ANO, NOT USUALLY INSISTED ON BY WRITERS ON THE SUBJECT.

I SHALL not occupy the time of this Association by dwelling on admitted facts in this disease. We all know that abscesses, having various cause and character, may form or exist in the adipose tissue, external to the rectum; but it is a matter of doubt and considerable

practical import, whether an abscess, being a simple phlegmon in its origin, and forming in the nates *juxta rectum*, is prone to penetrate the bowel, or even to denude it. I think it is not, for we see that abscesses arising from diseased lumbar spine rarely burst into the closely neighbouring bowel, and that abscesses of the liver usually tend outward. Is it in the usual course of natural process then, that a phlegmon in the adipose tissue of the nates should burst into the bowel rather than by an immediately external opening? The character of the structures do not warrant the supposition. No doubt, the nearer the bowel, and the more within the disturbing action of the sphincter, the more slowly, *ceteris paribus*, will an abscess heal; and it is likely, partially, to re-arise under such circumstances, and, perhaps, even more than once, but by-and-bye it heals permanently without operation. Where there is lesion of the bowel however, I believe, except in very recent and acute cases, the external openings never heal *permanently*, unless naturally substituted, or by surgical treatment. You may treat simple abscess near the sphincter, as is often done, by division, making its cavity one with that of the bowel, and it will get well; but so would this sort of abscess without division. The more diffused and frequently crepitant abscess, often seen near the anus, undoubtedly, is usually, perhaps always, connected with denuded bowel; its mode of origin in this part gives it *points* of hardness, but not the *circumscribed* hardness of simple phlegmon, nor has it quite the diffusion of erysipelatous abscess. I have never met with such an abscess in the neighbourhood of the anus, without clear evidence of disease in the rectum having preceded it, and that usually of many weeks' duration. Open the abscess, and, all agree, the sooner the better. In such a case you will find the bowel denuded; you may be fortunate enough now to find the inevitable opening into the bowel, but in a recent abscess it is not easy to do so, for there is nothing especial to point its exact locality. The lower part of the bowel all around will be found exquisitely tender, and corrugated deeply by violent action of the sphincters; however, the flaw in the bowel will nearly always exist a little to one side of its posterior mesial line, rarely an inch and a half high. In old-standing cases, a venous enlargement may indicate a likely point, but the ulcerated opening into the bowel is not *necessarily* on the side of the opened abscess; for there may be one on both sides, and there frequently is; and this I take to be the especial cause of such frequent failure to cure fistula by one operation. The ulcerated opening in the bowel is not unfre-

quently found on one side, and divided; or, if not found, it may heal there, by chance proximity of the cut; but the unhealthy pus has often run by a most easy, and usually unsuspected, track, in the loose tissue which pertains to the line of the sphincter, and has probably ulcerated into the bowel on its opposite side (this time the cause being from without) a little anterior to its back mesial line, just within its gripe. This secondary ulceration is more liable to be overlooked, as it is always unattended by those acute symptoms which mark the progress of the first, and this can be understood from the mode of formation; one begins in abrasion of the highly sensitive *within*, the other ulcerates from *without*. The opening, one or more, into the bowel being found, I need not say any mode of division is sufficient; that by knife, or the more painful and clumsy process of ligature. Undoubtedly in recent cases, division of the sphincter and denuded bowel will often cure, though the fistulous opening through the bowel has been untouched by the knife. It does so by quietude to the part, and adhesive inflammation extending sufficiently from the cut to block the neighbouring ulcer and radical mischief; and this good result not unfrequently occurring, the respective case is regarded as one of denuded, but not penetrated bowel. But what experience in the more difficult cases of fistula teaches, is, I think this, that if a case be of *long standing*, and the opening in the bowel has been missed by the knife, it will not heal; or if the internal mischief has been overcome on one side, it is still not unfrequently found that part of the external opening in the buttock refuses to heal, or heals and re-festers. Usually it keeps open, and by-and-bye sets up the faithful index of a fungoid granulation at the orifice of a fistulous track, which (starting with the condition of the bowel having got well on that side) is sure to run backward and upward in a crescentic direction, with the upper line of the sphincter. In all the several instances in which I have followed it, it has terminated in an opening just within the verge of the anus, on the opposite side, near its posterior part. As such fistulous tracks are neccessarily tortuous, the abutment of the probe is, of course, no proof of termination; cut to the end of that direction, and a new one can immediately be given to the instrument, and so on, till the opening in the bowel is arrived at: I have little doubt though, that in all ordinary cases, even chronic, cutting simply through the ulcerated opening in the bowel, *if you could be certain of doing this*, and including the sphincter, would be sufficient to cure the more external, but pertaining fistulous tracks. Some authors have re-

marked, as reason would indicate, that pain and tenesmus are more severe when the disease is near the sensitive outlet of the bowel; yet I have never found the secondary opening in the bowel, though even lower, highly painful in establishing itself, and, I believe, for the reason just now stated. I know, or at least it has been so stated, that some fistulous abscesses clearly connected with the bowel, establish an opening in the buttock with very little pain; these have had an internal opening in the sacculus recti, and, therefore, above the more sensitive parts. No such case has come under my treatment, but its occurrence is what may reasonably be expected, for it is only the very lower part of the bowel which is exquisititely sensitive—an endowment common to the other orifices. That most fistulæ in ano are difficult of perfect cure is undoubted, and clear from the history of its surgery. To cut out the "callosity," from the time of Celsus to Pott, was an unquestioned practice. This excising an inverted ∧ was done by Pott's great contemporaries, Le Dran and Boyer, and this failing, as it frequently did, to cure, the sphincter was often entirely dissected away. This must have settled the case. A severe and very questionable alternative undoubtedly, but one which proves *direct* that anal fistulæ were difficult of cure, and probably thought malignant, because of that difficulty. And it proves, by inference, that dissecting out the sphincter was a radical cure, because after it no mention is made anywhere of its recurrence. To be sure, the unhappy patient may have had enough of it, or it cured always, as the ∧ cured sometimes, by removing, partout, the internal lesion, which I take to be, almost without exception, the primal and essential cause. The confidently expressed opinion to the contrary appears only to have evidence to the extent that, fistula often gets well by a cut through the sphincter and bowel, when no opening into the latter has been discovered. We see how this frequently happens, and we see how it more frequently does not happen. Much attention was paid to this disease in the very earliest times of recorded surgery, and the comparatively modern writings on it of Faye, Le Dran, Cheselden, Boyer, and Pott, are more copious than those of our own times, and chiefly, no doubt, because of the dire operations thought necessary for its removal. No other man, has perhaps, paid so much attention to this disease as M. Boyer, he says, if my memory serves me, that out of three hundred cases, the particulars of which he witnessed or obtained, only two failed to show an opening into the bowel; but with all this, he did not establish a more lenient opera-

tion. Pott, the first English surgeon-philosopher—the antitype of Hunter,—appreciating the usually simple nature of fistula in ano, abhorred the unnecessary severity of the operations till then in unquestioned practice for its cure, and criticised them with the energy and force of a new idea; but new ideas are rarely calm, their antitheses repel them to extremes; so no sooner was the practice of bloodletting proved in excess, than reason was discarded in its fashionable overthrow; no sooner was excessive drugging made evident to the public, than they clamoured for infinitesimal globules; thus does the pendulum of opinion eternally rush to extremes, and tick first on one, then on the other side, truth. And experience proves that even Pott missed the rare medium when setting aside, for ever, the excisions, the horrible escharotics, the scoops, the wires, and medicated sponge tents. He delighted in his new idea of simple division, and truly, the distinctness of the process and its humanity were in pleasing contrast to what had preceded it; and he expressed his captivating theory with surpassing ability; but who is there, having much experience in the treatment of fistula in ano, not convinced that laying the external abscess open into the gut, will frequently fail to cure. That it often does cure is undoubted; and then, I think, by quietude of the sphincter, and extension of adhesive inflammation from the sides of the cut. It is not less certain, however, that a fistulous track often persists after the cut in the bowel has firmly healed; and such fistulous track I accept as clear evidence that some lesion in the bowel has been missed; find it, as you nearly always can, if not otherwise, by steady pursuit of the tortuous sinus; and, in ninety-nine times out of a hundred, it will prove within an inch or thereabout of the orifice of the bowel, near its back part; divide this essential lesion, whose origin was probably a fissure, or festered hæmorrhoid, by wending, *if you must*, through the tortuously approaching sinuses; but if you *can with certainty* divide it directly, so much is gained, and you will afterwards have no trouble with collateral affections: in opposition to the general disbelief or disregard of these particulars, there is the great authority of Sir B. Brodie, whose opinion corrobated his predecessor Boyer, that penetrated ulcer of the rectum is a never-failing condition of fistula in ano.

One observation more to conclude this rather lengthy paper. The internal opening having been effectually cured on one side by division, there are many instances in which a fistulous track will persist, and it will almost invariably lead backward from the cross-

cut in the buttock, by which the bowel was divided, and course, often zigzag, between the two sphincters, to the opposite side, and there open just within the gripe of the anus, about one-third anterior to its posterior mesial line. But as pus from the original abscess may pass readily between the two sphincters, and as ulceration of the bowel in secondary fistula takes place from without, instead of from within, there are usually neither acute pain nor callosity to mark its spot.

PERINÆAL CALCULUS.

H. R——, aged 75, of temperate habits, nervo-sanguineous temperament, twice married, and having a family, stated that fourteen or fifteen years before, he first suffered impediment to the free passage of urine; the difficulty steadily increased, and was caused, as he thought, by a general narrowing of the passage; no surgical treatment was adopted, and in time perinæal fistulæ formed. When he first came under my care urine dribbled freely through a large fistulous opening in the perinæum, and occasionally oozed through several smaller fistulous tracks, which dotted the tumified pubes; the sites of these lesser openings frequently varied, but the larger one in the perinæum was permanent. Up to the last, he passed daily a few drops of urine by the natural passage, but he could not be persuaded to submit to surgical treatment. In August, 1866, the calculus depicted in Plate 1 presented at the fistulous opening in the perinæum, and was removed without difficulty; in January, 1868, the patient died hectic; the larger fistula remaining open; a post-mortem examination was resolutely refused.

The calculus is four inches long on its convexity, and an inch and one-eighth in diameter at its thickest and most porous part, which, at the time of removal, lay at the neck of the bladder. It is of whitish colour, and light weight; its surface is chiefly porous, but in places it is rather dense and smooth. A section, shows it to have a nucleus near its centre, around which, oval striæ seem to mark stages of increase. It is composed of a large portion of animal matter, much triple phosphate, some phosphate

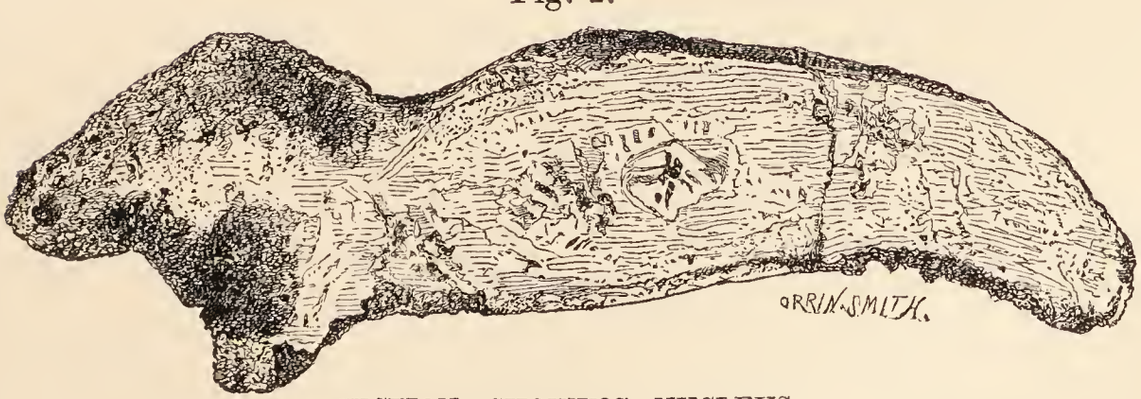
PLATE I.

CALCULUS REMOVED FROM A PERINÆAL FISTULA.

Fig. 1.

EXTERNAL ASPECT OF CALCULUS; NATURAL SIZE.

Fig. 2.



SECTION, SHOWING NUCLEUS.

of lime, and in places a trace of uric acid. A calculus, almost identical in shape and appearance, is in the Museum of the College of Surgeons, and is described in the Catalogue as "an unusual form of vesical calculus," but there is no history attached; both, probably, had a similar origin. Doubt of origin, is, in this instance, especially irritating, since post-mortem examination would have removed it; but in the absence of proof, I infer, as well from the section as the composition of the calculus, that it escaped while small from the prostate, and that as the urine trickled through the perinæal fistula, it lowered in temperature, becamed mixed with purulent matter, and stalactited around the thus increasing mass.

BRITISH CHOLERA AND ASIATIC CHOLERA CONSIDERED IN CONNECTION WITH THE FUNGOID THEORY, AND THEIR TREATMENT BY CASTOR OIL.

By Spencer Thomson, M.D.

Twenty-two years have elapsed since, in the summer and autumn of 1846, we experienced a season similar to that which passed over us in 1868. The heat, in the former period, was not perhaps so uniformly intense as in the latter; but, according to the Report of the Registrar General, which followed in due time, the three months ending September 30th,* 1846, were characterised by a startling increase of disease and mortality. The report alluded to was taken from 115 districts of England, 34 of these being metropolitan; the remaining 81 comprised the principal towns and cities, and some agricultural districts. In these districts, numbering a population approaching 7,000,000, the deaths registered during the quarter ending September 30th, exceeded the deaths of the corresponding quarter of 1845 by more than 15,000. In London alone the deaths exceeded those of the summer quarter of 1845 by 1567, and of that excess 1303 were from cholera, diarrhœa, and dysentery. The mortality from intemperance, delirium tremens, jaundice, and other liver diseases, and from rheumatism, was also greater than usual. Almost universally throughout the kingdom there was an increase of death, and in some of the large towns the mortality was nearly doubled. Here then we have evidence that, coincident with a hot, almost tropical season in the generally temperate climate of Britain, there occurred a remarkable increase in the amount of illness and death; that increase being

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* The average mean temperature in the metropolis for the quarter ending September 30th, 1846, was 63·1 deg., whilst in the corresponding quarter for 1845 it was 57·1 deg. See Report.

due almost entirely to one special class of diseases, namely diarrhœa, simple or accompanied with bilious vomiting, (when it is usually named British or bilious cholera,) and others closely connected with disorders of the hepatic system.

Let us now compare with the above some of the reports published during the hot summer of 1868. It will be sufficient to take, without special selection, two of those issued during the month of July. The first is for the week ending Saturday, July 11th. "The mean temperature for the week was 65·7 deg., which is 3·9 deg. above the average of the same week in 50 years (as determined by Mr. Glaisher). The highest day temperature was 85·0 deg. on Thursday, 9th July." For the week ending July 25th, the report is as follows:—"The mean temperature of the air was 69·2 deg., which is 7·6 deg. above the average of the same week in 50 years (as determined by Mr. Glaisher). The highest day temperature was 96·6 deg. on Wednesday, July 22nd. This is a higher value than has been recorded at the Royal Observatory as far back as authentic record extends." But, in the above Reports, from "The Registrar General's Return," we find that the deaths in London for the week ending July 11th exceeded by 126 the estimated average of previous years, and of the whole number of deaths, amounting to 1,505, registered for the week, 319 were from diarrhœa and cholera. For the week ending July 25th, when the heat had greatly increased, the number of deaths had also increased, and as many as 1885 were registered, exceeding by 310 the estimated average, and by 243 the number recorded in the preceding week. Of the 1885 deaths which occurred in London during the week ending July 25th, just 500 were registered as due to "diarrhœa" and "summer cholera." In a leading article in the "Lancet" for September 19th, 1868, it is stated that the total registered deaths in London from diarrhœa and ordinary summer cholera in the thirteen weeks from the beginning of June till the end of August, amounted to 3,302. Another report gives the deaths from diarrhœa in London during the summer of 1868 as 3,145; in the summers of 1867 and 1866 respectively, the deaths from the same cause were 2,186 and 2,298, in the latter year cholera being epidemic. In the cities and large towns of England, diarrhœa was even more prevalent and fatal than in the metropolis. It is needless to multiply proofs of a fact which is now generally admitted, that a continuation of elevated temperature, and the occurrence of diseases connected with the liver and its functions, stand to each other in

the relation of cause and effect. To quote the words of Mr. Annesley, "The influence of the direct rays of the sun and a high range of temperature in producing hepatic derangement cannot be doubted." It is stated that even dogs taken from Europe to Southern India, suffer from the severest form of liver disease.

In the year 1848 I published a small work upon "British Cholera," taking for my basis the report, above quoted, of the Registrar General upon the hot summer and autumn of 1846, and, at the same time, the records of my own practical observation of many cases which came under my care during the period in question. More than twenty years' experience has only served to confirm the views I then took of the nature and causes of summer cholera, and of the hepatic derangements which seem necessarily to arise even in the heat of average summers, and to be notably increased whenever the heat rises much above the ordinary standard. Two questions arise. First—Why this should be? Second—By what means are these injurious and fatal effects of heat to be alleviated, if they cannot be entirely prevented?

In answering the first, it is necessary to get rid of the idea that the prominent symptoms, the purging, vomiting, and their concomitants, which most naturally attract the attention, constitute the disease British or bilious cholera, instead of being only the terminal stage, in which the natural effort of the system is to free itself from morbid accumulation which has been taking place during the initiatory stage, giving rise to characteristic and well marked symptoms. In illustration, the best method will be to trace the history of the epidemic bilious cholera of the summer and autumn of 1846, as observed in private practice. Very soon after the commencement of the hot weather, early in June, a succession of cases, marked by peculiar, but very distinct symptoms, began to present themselves for treatment. The most prominent complaints were those of general feeling of lassitude, and inaptitude for exertion, physical or mental; depressed spirits, and headache, especially after meals; sleep, disturbed by disagreeable dreams, or very heavy, and *constant insuperable drowsiness in the daytime*, particularly after meals; gastric and hepatic functions much deranged; bowels irregular, occasionally griped; urine generally depositing highly coloured urates and uric acid. In females, the catamenia much darker than common, and more offensive; pulse often slower than natural; limbs aching on slight exertion; skin dusky, but not yellow. The cases characterized by the above

symptoms were all of them readily cured by appropriate treatment. They all, however, presented one peculiarity, the readiness and frequent violence with which simple mercurial medicines acted as purgatives, even in minute doses, with immediate relief to the patient. These remedies, in fact, produced an artificial cholera, varying in intensity according to the duration and amount of the premonitory stage. The slightest cholagogue stimulus proved sufficient to occasion the evacuation of a quantity of ill-conditioned bile from the previously overburdened liver. Dr. Copland, in treating of bilious diarrhœa, mentions this effect; he says—"An aperient or purgative medicine may even excite it (diarrhœa) if the biliary organs be loaded at the time with morbid or acrid bile." * It may here be remarked that whether or not mercurials have power to increase the secretion of bile in the healthy liver, they have certainly energetic action in promoting its evacuation, when it, or its constituents are present in undue proportion in the liver, or possibly in the system generally.

As the hot summer of 1846 advanced, cases presenting the above symptoms became more and more frequent, and many persons who did not seek for timely medical relief, were at last forced to do so, when, after enduring the premonitory disorder for a longer or shorter period, it terminated in an attack of bilious cholera or diarrhœa. Frequently these attacks were mild, the constitution was relieved by the discharge, as if it had been caused by medicine, and health was restored; at other times this salutary discharge being arrested, either artificially or naturally, and partial relief only being obtained, the symptoms continued, at first with abated, but afterwards with increasing intensity, till another crisis partial or effectual ensued. The mild attacks of diarrhœa were characterized chiefly by the passage of, at first, dark bilious stools, with more or less griping, the stools becoming lighter in colour, and more natural, and the griping ceasing simultaneously. Between this mild form, and that in which the disease assumed almost the intensity and symptoms of malignant cholera, cases of every degree presented themselves. In some instances the biliary soon gave place to the rice-water evacuations; collapse, suppression of urine, intense thirst, and cramps quickly supervening. In a few instances, towards the close of the hot season of 1846, the hepatic disorder took the form of active irritation, if not of sub-acute inflammation

* Art. "Diarrhœa." Dict. Pract. Med.

of the liver, with fever, and urine loaded with deeply purpuric urate of ammonia. Although the above brief outline of the symptoms of the bilious or British cholera, which usually accompanies and succeeds a hot season in this country, is drawn chiefly from the epidemic of 1846, it applies equally to that of 1868, and will serve equally well as a basis for the deductions which follow in the remainder of this paper. These deductions, moreover, being drawn from the comparatively overlooked, but more important, indications of the initiatory stage of the disease, rather than from the more prominent symptoms of its conclusion; a conclusion which I am certain might, in the majority of cases, be greatly modified, if not avoided altogether, by proper attention to diet and to habits of daily life, especially on points where that diet and those habits, however well suited to our ordinary cold or temperate conditions, become entirely unsuitable when we are surprised by almost tropical seasons, such as those of 1846 and 1868. It requires little demonstration to prove, that between the carbon introduced into the body in the food, and the oxygen introduced by the lungs and skin, some sort of proportion or balance must be sustained; disturb that balance beyond the compensating power of the system, and disorder or disease is the consequence. But the quantity of oxygen inspired is affected by the temperature and density of the atmosphere. Dr. Copland says, in his article on "Disease," "It has been shown by the experiments of Prout, Fyffe, Allen, and Pepys, in an artificially increased temperature, and by those I made in an intertropical atmosphere, that heat remarkably diminishes the changes effected by respiration on the blood." Even this inactivity and indisposition to exertion, produced in many persons by a heated atmosphere, aid in diminishing the supply of oxygen. Under this diminished supply should the same amount of highly carbonized food and drink be thrown into the system as during cold weather, the carbon elements cannot be consumed, and must necessarily accumulate. The blood, half purified, circulates slowly, and the whole train of symptoms which have been enumerated as constituting the first stage of the disease under consideration, and as heralding the approach of the second stage—that of vomiting and purging—is more or less quickly developed. These symptoms are all highly characteristic of blood loaded with carbon. The torpid nervous system, the *overpowering drowsiness*, the headache, the dusky skin, the high-coloured urine, the dark catamenia, and the digestive organs disordered by and sympathising with a

liver oppressed by its additional duties. The effects, both physiological and pathological, of poisoning by carbon gases, the effects of excessive cold, the symptoms of jaundice and of cyanosis, the peculiar drowsiness and leaden sleep of anæmia, the drowsiness of travellers who have ascended great heights, may all be given as evidence of the effects of blood, from which the carbon element is not sufficiently eliminated—*all partake of the insuperable drowsiness of the first stage of British cholera.* Assuming the above position to be correct, it is evident how much a matter of common sense and of personal management the avoidance becomes of that physical condition, which, initiating in carbon-laden blood, and oppressed hepatic functions, terminates at last in the really natural effort to free the system, which culminates in bilious diarrhœa, alone or combined with bilious vomiting, and in its severest forms, with cramps, rice-water stools, suppression of urine, and collapse.

The hygienic management, either of ourselves or of our patients, during an unusually hot season, is tolerably plain.

Attention to the condition of the skin by frequent ablution, and free ventilation of rooms, especially at night, is of course most important, as well as no more exposure to mid-day heat than is absolutely requisite; but it is on the diet most care has to be bestowed. Nature indeed gives us some aid in the matter, for all know how, under continued hot weather, the foods and drinks of the colder seasons become distasteful to a naturally healthy appetite. The meats and fatty articles of diet that agree so well and are relished so well during winter, and which do so much to sustain life and health in arctic regions, are now to be avoided, and in their place the farinacea, fruits, vegetables, and light wines substituted. And here I would more especially revert to that mistaken view of the nature and causes of British cholera, and of biliary disorders, which has led not merely to the disuse, in obedience to popular prejudice, of fruits and vegetables as conducive to choleraic attacks, but to their prohibition by medical authority, instead of their being regarded as the providential provision for the hotter seasons of the year. Undoubtedly the consumption of fruit in unmeasured amount, or of fruit either unripe or decayed, will produce disorder, and, should the system be already tending to it, may *seem to be* the direct cause of an attack of British cholera, by inducing increased or disordered action of the digestive organs generally. But this is a very different thing from the regular moderate use of mature sub-acid fruits, and of wholesome vegetables, which really act as prophylactics.

If to the common-sense hygienic rules given above we add the occasional administration, when any of the initiatory symptoms of the disease show themselves, of gentle cholagogue and other aperients, it may safely be said that the numbers attacked by this painful, and, at times, fatal disease, may be greatly diminished.

As regards treatment when the actual outbreak has occurred, either of diarrhœa alone in greater or less severity, or of diarrhœa with vomiting, the remedial measures may be divided into four classes. The simple diluent or soothing treatment. The aromatic and astringent, with or without opium. The chemical, that is by means of dilute sulphuric acid, which acts, probably, by altering the irritating properties of the acrid and superabundant bile poured into the alimentary canal, the acid being given either with or without sedatives. And, lastly, the eliminative or aperient, chiefly castor-oil, treatment.

The simple diluent or soothing treatment of bilious diarrhœa is, of course, applicable to those mild cases in which natural effort is doing nearly all that is necessary to free the system, the indication being to dilute the unhealthy secretions so as to render their passage through the bowels easy and unirritating. Even in these cases, however, it is generally requisite to close with a dose of castor-oil, or of some mild rhubarb aperient.

The astringent and aromatic treatment, represented by the old chalk or bismuth mixtures with aromatics of various kinds, with rhubarb, catechu, kino or opium, as the case might be, has now been greatly superseded by the much more effectual, and certainly much more agreeable, sulphuric acid treatment. There are, however, certain cases, to which the old treatment is still specially applicable; those in which diarrhœa, either acute or sub-acute, has been going on for some time, and in which, consequently, the mucous membrane has become chronically irritated, the tongue having become morbidly red, or with red tip and edges. In such cases, the alkaline earths, bismuth, with astringents, opium, and small doses of mercurials are most useful, accompanied with turpentine stupes, or sinapisms with laudanum externally. In the early acute stages of bilious cholera or diarrhœa, however, the dilute sulphuric acid treatment has almost unanimously been adopted by the profession, because, no doubt, of its immediate and marked remedial effect. Even when given alone, frequently repeated doses of the dilute acid are most beneficial; but, unquestionably, in some cases, the addition of an aromatic, such as the tinct. cardam co., which at

first was generally prescribed with it, or opium with chloric ether, or, still better, chlorodyne, is most serviceable. Any one who had previously taken the thick distasteful chalk mixtures, and in some subsequent attack been treated with the clean tasting sulphuric acid, would feel almost instantaneously that the latter was the better, as well as the most agreeable remedy. It is difficult to explain the remedial power of dilute sulphuric acid in bilious diarrhœa, except by its chemical action on the bile itself, without which its astringent power would avail but little.

As regards the castor oil or eliminative treatment, so authoritatively introduced as a remedy, in Asiatic cholera especially, I simply quote from my work, published in 1848, "If vomiting is not present, or if it has been subdued, and if the purging is excessive, or any sign of muco-interite come on, there is no medicine which can be given with such never-failing success as castor oil, in the form of emulsion made with yelk of egg and distilled water, simple or aromatic, and combined with opium."

To sum up, I should say that the simple or diluent treatment simply aids natural effort to throw off the morbid bile as easily as possible.

The astringent, aromatic, or opiate treatment, in the severer cases, restrains violent and excessive action, quiets pain and spasm, allays mucous irritation, and permits the more easy and gradual removal of the morbid contents of the alimentary canal. Often, however, this treatment proves insufficient, and requires to be followed by aperients.

The dilute sulphuric acid treatment acts, probably, by neutralizing the morbid, and at the same time the irritating properties of the bile, and permits it to be more easily passed off.

The castor oil acts by its eliminative power, its easy action being increased by combination with stimulants, aromatics, and opium.

The above four diverse modes of treatment all tend to cure the patient, but in different ways. Be it observed, however, that the castor oil treatment is more especially applicable to British cholera; whether it is equally applicable to Asiatic cholera remains to be seen.

On the question of the castor oil treatment of cholera, whether British or Asiatic, more, perhaps, hinges than might at first sight appear. If, as I have attempted to show, the disease called British or bilious cholera is specially owing to an undue accumu-

lation of carbonaceous matter in the system; and if its concluding stage, manifested by purging, vomiting, &c., be simply the natural, though painful, and sometimes dangerous efforts of the system to free itself from that morbid accumulation, through the emunctories best suited by their functions to accomplish this end; then may we understand how castor oil, by its complete, yet soothing action upon the bowels and their mucous lining, is a most valuable aid in the treatment. But if, on the other hand, the Asiatic or malignant cholera be, as it has always seemed to me, and as it is considered by many high authorities who have had ample opportunities of observing it in its worst Indian forms, a disease originating primarily in, or at least distinctly traceable to, the ganglionic or cerebro-spinal system; it is difficult to see in what way the castor oil or eliminative treament can prove of much benefit. In the foregoing pages much stress has been laid upon the fact that British cholera is preceded by a well marked initiatory stage of longer or shorter duration. It is one of the distinctive features of the Asiatic disease, that it very often occurs without any warning whatever, striking down persons apparently in the most perfect health. Dr. Aitken remarks, "The remote cause of this disease is unquestionably a poison; for at no period has a person in good health, in this or any other country, been known to become in a few minutes shrivelled up, his whole body to be of icy coldness, his face and extremities to turn purple, and, with or without vomiting of a peculiar fluid like rice water, to die in a few hours, except under the influence of a poison."* In the "Petersburgh Report," made by Drs. Russell and Barry, in 1831, they remark, that owing to the absence of premonitory symptoms, their means of observation of the first features of the disease were very limited. Mr. Shaw, who was surgeon to the 86th regiment during the Kurrachee epidemic of 1846, says, "In three-fourths of the cases the attacks were sudden, often within a few minutes, in the other there were, occasionally, precursory symptoms." In many instances death took place in two, three, or four hours. Indeed this sudden invasion, and rapidly fatal termination of Asiatic cholera is too established a fact to require further confirmation. Equally well established is it that cramps, vomiting, purging, &c., are not essentials of the disease, but simply superadded effects, the most fatal cases never presenting these symptoms

* "Practice of Medicine," 4th edition, p. 634.

at all. "A mortal coldness, with arrest of the circulation, coming on from the beginning, and the patient dying without a struggle within three or four hours." * In the above cases death is likened by many of the writers to that resulting from some sudden concussion of the nervous system. Indeed, Dr. Aitken states that "when Asiatic cholera reached Muscat, instances are given in which only ten minutes elapsed from the first apparent seizure before life was extinct."

Thus we find those symptoms which have been most prominently dwelt on, from which the two diseases have taken their common name, are non-essential to either, but from very different reasons. In British cholera, actual vomiting and purging may not occur at all, the carbon accumulation may go but to a small extent, and the cause ceasing to act, the *materies morbi* may be imperceptibly discharged. In Asiatic cholera we have just seen that choleraic symptoms, properly or generally so called, may be entirely absent. Considering then, the vomiting, purging, and cramps, and even the algide symptoms, to be the effects merely of some prior cause, the rapidly fatal cases above alluded to, must be regarded as the purest form of the disease. The symptoms in these cases are so indicative of a nervous system affected in a manner peculiar and inexplicable, that enquirers have naturally been led to look to that system for an explanation. Among the early investigators, M. Keraudren says, "The first stage of this malady appears to be essentially nervous or spasmodic." Dr. Ainslie considered that "the nervous influence received the first morbid impression in cholera." Mr. Twining's evidence tends to the same point; he says, "In an assemblage of those symptoms which constitute the early stage of a sudden invasion of cholera, we observe evidence of the disorder or total cessation of the functions of those organs which are supplied with nerves from the great solar plexus;" and again, he says, "The fatal termination of cholera in some of the most sudden cases, commencing with extreme collapse, seems to depend on the intensity of the efficient cause of the disease, acting so powerfully on the nervous system as to produce total arrest of all vital energy." Dr. Kennedy considered cholera to be "an affection of the nervous system, similar to that produced by concussion of the brain." Mr. Shaw, in his account of the cholera at Kurrachee, remarks, that "the pure pathognomonic

* Dr. Brown, "Cyclopædia Pract. Med.," Art. Cholera.

symptoms of the disease as we met it were loss of nervous power." Lastly, Dr. Brown, in the "Cyclopædia of Practical Medicine," adds his testimony to the above, saying, "That the nervous system generally, and especially the ganglionic and spinal nerves, and the spinal medulla itself, are affected, is manifest from many symptoms." Led by the symptoms of the disease to trace back its origin to the nervous system, enquirers have endeavoured to verify their opinions by post-mortem investigations, and many of the results have been recorded. Lizars and Delpeche found the brain, spine, and ganglia affected in many of the cases examined by them during the epidemic of 1831-32. Dr. Davy, in Ceylon, found the nervous centres congested. Dr. Keir, of Moscow, observed in Russia, the blood vessels of the brain and of its membranes more or less turgid with blood; also the blood vessels of the vertebral column and of the spinal cord more or less loaded with blood, which was sometimes effused between the arachnoid and dura mater. Dr. Parkes, in his work on Cholera, says, "That the most usual appearances in the head, consist in the accumulation of blood in the veins of the dura and pia mater, and in the effusion of serum or blood consequent upon this." The congestion was considerable in the most malignant cases. Dr. W. T. Gairdner found the membranes of the brain and cord in general congested, and the substance of the brain clotted with more puncta cruenta than usual. Certainly the above pathological observations, when taken in conjunction with many of the symptoms of malignant cholera, lead us to the nervous centres, or at least to the spinal and ganglionic systems, as the points from whence the manifested symptoms of the disease first start, but the primary cause is left unexplained. Some writers have endeavoured to trace this primary exciting cause to peculiar electrical or ozonic conditions of the atmosphere; but the latest investigations, tending, or at least attempting, to demonstrate the fungoid origin of cholera, derive at the present moment additional interest from the able report of Drs. Cunningham and Lewis, published in the "Lancet,"* in which they communicate the results of their mission to Germany to "receive special instruction on the methods of investigating the forms of fungi." Their account of the scientific work in this field of Professors De Bary and Hallier, and of Professor Von Pettenkofer of Munich, is especially interesting.

The probability of the spinal and ganglionic systems being the

* January 2nd, 9th, 16th, 1869.

chief centres whence the symptoms of Asiatic cholera originate, is not at all incompatible with the fungoid origin of the disease; not, however, as some have supposed, or as Professor Hallier seems to indicate, by direct action of the morbific agent upon the alimentary canal, or as a blood poison changing the vital composition of that fluid, but as a *specific poison acting upon the nervous system.* We find at least one fungoid production, the ergot of rye, acting thus specially upon the nerve centres; causing, in some instances, convulsive ergotism, in others the well known dry gangrene of ergot; both affections being recorded to have, at times, taken the form of epidemics in districts where rye is the usual food of the people. Further, although the secale cornutum was for long used almost solely as a uterine stimulant, without any clear idea of its *modus operandi*, its special action upon the cerebro-spinal system is now a well ascertained physiological fact, and, consequently, its therapeutic value is daily becoming more appreciated. In a recent paper by Dr. Alfred Meadows, upon the "Therapeutical Uses of the Ergot of Rye," he observes,* "There is abundant evidence to prove that ergot of rye influences very powerfully the cerebro-spinal system, and this it does apparently through the medium of the blood vessels. Brown Séquard observed that the vessels of the pia mater of the dog became much smaller under its influence, and further that the reflex action of the spinal cord was considerably diminished. Hence he was led to employ this remedy in cases of chronic congestion or inflammation of the spinal cord and its meninges, and obtained, he says, "results greater than he had even dared to hope." From its power of influencing the circulation of the blood in the small capillaries, ergot has become one of our most valuable remedies in the treatment of pulmonary hæmorrhage. Its use in neuralgia is gradually becoming more extended, and it seems specially applicable to that form which accompanies the eruption of shingles, the eruption and the pain being both considered, by some, to depend upon the loss of nervous control over the cutaneous capillaries. Now, if one fungoid production can exert such marked toxical and therapeutical effects in the human organism, it is not irrational to suppose that another fungoid production (taking it for granted, of course, that the fungoid theory is the true one) might also produce marked toxical effects, though of a different, and even opposite character. Thus, if the fungoid growth deve-

* "Practitioner." No. iii.

loped on rye originates convulsive disease and dry gangrene in man, and the casting of the hoofs and abortions among cattle; and if, moreover, it has a direct physiological action upon the cerebro-spinal system, such action causing it to exert either an injurious or beneficial effect upon the human body, according to circumstances; may not another fungoid growth, developed upon rice or some other grain, have *its* special action upon the nervous system, and thence upon the body generally, producing the fearful and too often fatal symptoms which go to make up the disease usually called Asiatic cholera? Fortunately, the fungoid growth on rye seems incapable of perpetuating its existence apart from its nidus upon that or some other cereal. The cholera cyst or germ, whencesoever derived, requires, according to the views of Professor Pettenkofer, certain conditions to "bring about an epidemic of cholera. These are certain local conditions—certain seasonal conditions—certain individual conditions." The existence of such a germ, and of such conditions provided for its development, afford, according to the Professor, the only, or at least the most probable, explanation of the phenomena attending the diffusion of Asiatic cholera. According to Professor Hallier, schizosporangium, or cholera cyst, is peculiar to the disease in this climate, and can only be "developed on a nitrogenous basis, and under a high temperature."

Of course much of the foregoing line of argument can only, in the present uncertain state of our knowledge, be suggestive; but if investigators are now on the right track, or if the disease Asiatic cholera be proved to have a fungoid origin, I think it will be found that the symptoms arise, not from irritation thereby initiated in the alimentary canal, whether direct or reflex, but from direct irritation of the cerebro-spinal, and probably the ganglionic systems, caused by the toxic property of the fungoid development being conveyed to those nerve centres through the blood vessels. Should such be the case it will show how vain must have been the efforts of science to find the *fons et origo* of the disease by examinations of the blood and excreta, *further than as these are affected through the prior cause.*

The fact that excretion from various organs may be vitiated, immoderately increased, or the reverse, by causes acting through the nerve centres, scarcely requires illustration. Electrical disturbance and mental emotion will cause profuse diarrhœa; melæna has been said to result from violent passion; the kidneys transude enormous quantities of fluid in hysteria. Bernard's experiments proved that

irritation of certain special points in the brain produced exaggeration of the hepatic (saccharine) secretion, and also exaggerated secretion of urine, the irritation being conveyed by the spinal cord and filaments of the great sympathetic to the liver.* The excretion of bile is at all events interfered with by mental emotion, and jaundice results. Lastly, the excretion of the skin, the cold sweat of fear or of faintness, probably comes under the same category.

It is probable that the sweating sickness of the sixteenth century may have been ascribable to some cause analogous to that which originates malignant cholera, in which, instead of being determined to the mucous lining of the bowels, the fluid passed off by the skin; many of the recorded symptoms of the epidemic closely resembled cholera. In illustration may be cited a sporadic case of sweating sickness, reported by Dr. Laurie,† as having occurred in Glasgow, the symptoms of which accord with this view, which is rendered still more probable by the circumstance that the profuse sweat of the algide stage of malignant cholera sometimes becomes the most prominent symptom, as in the Kurrachee epidemic.

If then Asiatic cholera be a disease in which the nervous centres, whether affected by the fungoid poison or not, are also the centres whence the characteristic symptoms of the disease are radiated, it certainly would seem that the true remedies are to be sought amid those therapeutical agents which act most directly upon these centres, and which can be introduced into the system in such a way as to act most quickly and certainly. It is scarcely needful to remark, that, in a disease where the powers of absorption from the alimentary canal are almost for the time annihilated, it is all but futile to give remedies by the mouth, and that subcutaneous injection must offer the best mode of administration. Ergotine, atropine, and the various other nervine remedies, will probably be selected according to the judgment of the practitioner. Of course it is not meant that reliance should be placed in these neurotic remedies solely, though it is probable they may prove the true curative agents; but, along with them, other remedial agents must undoubtedly be of use. Ice bags to the spine, the free imbibition of water, cold or tepid, to quench the distressing thirst, abundant swallowing of ice, and saline injections into the veins, may all, and each, according

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* Ext. "Aitken's Practice of Medicine," vol. ii., p. 133.

† "Journal Med. Science," vol. vii.

to the experience of numerous practitioners, be employed with benefit.

In conclusion, to revert to the often repeated comparison between British and Asiatic cholera, I would direct attention to the fact that in the former, when algide symptoms come on, it is not until the profuse diarrhœa and rice-water stools have been going on for a considerable period; the algidity, if I may use the phrase, being probably due to the draining off of the serum of the blood. In the latter, algide symptoms may come on from the very commencement of the attack, and the patient may die in the algide stage, without either vomiting or purging. In short, I trust that the arguments used in the foregoing pages go a long way to prove that the two diseases, British cholera and Asiatic cholera, are related more in name, and in a general resemblance of their ulterior symptoms, than in anything else; these ulterior symptoms of resemblance being chiefly manifested upon the mucous tract of the alimentary canal.

In support of the views respecting the exciting cause and development of the symptoms of Asiatic cholera, mooted in the foregoing pages, I may cite the undoubtedly increasing tendency amongst medical observers to trace back the primary, or, at least, the acting causes of many diseases to the great nerve centres; even of diseases which have hitherto been thought dependent upon local conditions, or traceable to the blood and its changes. Amongst others, in an able paper* in the "British Medical Journal," for March 13th, 1869, Dr. Eade, of Norwich, moots the possibility of pulmonary phthisis taking its rise in cachectic or predisposed individuals from partial or irregular exhaustion of the nerve centres, by which "their influence on the pulmonary membranes is cut off only in spots or localities; and then, where this takes places, the irregular vital action which results, eventuates in the production" of ordinary tubercles. Again, Dr. Eade says, "In cases of phthisis, originating in mechanical irritation of the lungs by flour, grinder's dust, &c., the exhaustion is somewhat of a different character. In this case the peripheral extremities of the nerves of the pulmonary mucous membrane would be the parts primarily exhausted of their force, and the effect would be exerted either directly or reflexly." Should these views be correct they would support the idea that the

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* Read in the Medical Section, at the Annual Meeting of the British Medical Association, in Oxford, 1868.

algide condition, and its accompanying symptoms, might result in Asiatic cholera from causes acting primarily within the nerve centres, or in British cholera, partly at least, from reflex irritation transmitted from the intestinal mucous membrane.

In brief language, the British disease first exhibits its symptoms at the periphery, and develops, as it were, towards the centre; the Asiatic, commencing at the centre, develops its symptoms outwards.

I need scarcely add that in the foregoing brief paper very many points of interest connected with both diseases have been left unnoticed, my aim having been, as far as possible, to confine myself to those which bear directly upon my argument.

ON THE INFLUENCE OF A MOIST ATMOSPHERE IN THE PRODUCTION OF PULMONARY CONSUMPTION.

By Edwards Crisp, M.D., M.R.C.S., L.A.C., late Physician to the Metropolitan Dispensary, &c.

In a paper which I had the pleasure of reading before the first meeting of our Association, on tuberculous affections in man, and in the lower animals, in relation to their supposed zymotic nature, ("Transactions," vol. 1, page 110,) I gave the proportion of deaths to the population from phthisis in the metropolitan districts, and in fifty-four of the chief cities and towns of England and Wales. I excluded Scotland, because in the Scotch returns the objectionable practice is adopted of including tabes mesenterica and hydrocephalus with phthisis. From Ireland, at that time, I could obtain no satisfactory returns. In this communication I stated, as the result of my investigations in England and in other countries, that *a dry* atmosphere, hot or cold, was the most favourable, and that those living in such an atmosphere were less liable to be affected with this fatal disease.

Having the examples of the tubercular animals at the Regent's Park Gardens before me, a district with a cold dry impermeable soil, and often enclosed in fog, as I mentioned in my paper on the causes of death of these animals dying at these Gardens from 1851 to 1860, I had not proceeded far in my research before I discovered that such an atmosphere was highly injurious to those predisposed to phthisis. I was especially struck with the vast difference that existed in various parts of England and Wales, and indeed in the metropolitan districts respecting the comparative prevalence of this disease, in some places the mortality being treble, and even quadruple, that of others. I was particularly surprised at the difference between Hampstead and Chelsea. In the former

locality, as stated in my last paper, during ten years, only one person in sixty-one dying of phthisis, whilst at Chelsea the death-rate from this cause was one in twenty-eight.

I was then induced to extend my inquiries to the 623 districts of England and Wales, taking into account the nature of the soil, height above the sea level, state of the drainage and the population, occupation, and food of the people. In addition to these I have placed before the meeting a map that I have formed of England and Wales, in which I have given the death-rate from consumption in each district; the amount of population, as well as the number of deaths per 1000 from all causes, during twenty years, (1840 to 1861) from the Registrar General's Reports. I have also noted the presence of hospitals and barracks, establishments that may often serve to swell the mortality. At the last meeting of the British Association at Norwich, it was my intention to have read in the Statistical Section a paper on the statistics of Pulmonary Consumption, in the 623 districts of England and Wales; but the communication was deferred until the last day of the session, when, in consequence of the number of papers to be read, I was enabled only to give an outline of the subject. A very short abstract of this communication is published in the "Medical Times and Gazette," 1868, vol. 2, pages 279 and 352; and also in the 'Transactions" of the Association, 1868.

When I read my paper last year I was not aware that any one had previously pointed out the injurious influence of a moist atmosphere in the production of phthisis. I knew that Dr. Wells in this country, Baudin in France, in his *Traité des fièvres intermittentes*, 1842, and several other French writers about that period came to the conclusion "that in aguish districts pulmonary consumption was less prevalent; that the one affection was antagonistic to the other," one of the many delusions that has been palmed upon the profession from want of proper statistical evidence, but I shall return to this subject hereafter.

I find that Dr. Bowditch, in 1862, read a paper before the Massachusetts Medical Society, on moisture of the soil as one of the chief causes of consumption in New England, where he found what he calls many "consumption-breeding districts."

Dr. Gross, of Berne, in the *Journal de la Societé de Statistique* de Paris, 1867, in his paper entitled "Repartition geographique de la phthisie pulmonaire," states that all countries where phthisis is prevalent are remarkable for being more or less damp or moist.

Dr. Gross appears to have obtained his information chiefly from the writings of others and by the collection of published statistics, quoting from Dr. Hérsch's *Pathologie Historique*, he gives the reputed mortality from consumption in the following capitals of Europe and America, although I scarcely need say the figures are but little to be relied upon, as the investigators must have trusted greatly to their imaginations. The numbers indicate the proportion of phthisical deaths to 1000 of the inhabitants.. Copenhagen, 3·4 ; London, 3·7 ; Edinburgh, 4·8 ; Glasgow, 7·0 ; Paris, 4·1 ; Malta, 3·3 ; England, 3·0 ; Dresden, 3·0 ; Wiesbaden, 2·9 ; Wurtzburg, 5·7 ; Brunswick, 2·9 ; Bavaria, 3·7 ; Algiers, 2·9 ; St. Helena, 2·2 ; New York, 5·3 ; Philadelphia, 5·6 ; Baltimore, 4·0 ; Boston, 3·8; Charleston, 3·7 whites, 4·0 negroes; New Orleans, 3·7 whites, 4·1 negroes ; St. Louis, 3·5; Memphis, 5·0 ; New Jersey, 1·7 ; Massachusetts, 3·0 ; Berne, 4·5.

I quote these statistics, although I attach but little value to them.

Dr. Bowditch, during the present year, 1868, has again brought the subject before the Massachusetts Medical Society, and his subsequent experience in New England has confirmed the correctness of his first conclusions ; he finds many wet, consumption breeding places in New England, were phthisis is very prevalent, whilst in drier localities it is comparatively rare.

Since my paper was read, December, 1867, Dr. Buchanan, in the tenth Report of the Medical Officers of Health of the Privy Council, 1868, has made a report on the distribution of phthisis, as affected by dampness of soil. Dr. Buchanan's researches extend only to the south eastern counties of Kent, Surrey, and Sussex ; these include 58 registration districts, having a population of 1,118,372, living on 3,812 square miles. These are the only counties in which a minute geological survey has been made. The geologists associated with Dr. Buchanan were Messrs. Whitaker and Topley. Dr. Buchanan concludes that wetness of soil is a cause of phthisis to the population living upon it.

Dr. Buchannan excludes all cases of phthisis in persons under fifteen years of age, as well as those above sixty.

It appears to me that such an exclusion as this must greatly invalidate his conclusions. Of the 4,210,715 deaths in England and Wales from 1851 to 1860, 508,923 were from phthisis ; and I find that 135,248 were of the age of 15 or under, and over that of 55. Under 5 years of age, 32,945 ; 5 years, 12,695 ; 5 to 10, 20,610 ;

10 to 15, 54,620; 15 to 20, 73,030; 20 to 25, 123,117; 25 to 35, 90,359; 35 to 45, 56,247; 45 to 55, 30,922; 55 to 65, 12,250; 65 to 75, 1,961; 85 and upwards, 168.

The objections I make to Dr. Buchanan's conclusions are the limited extent of his survey; the exclusion of the ages mentioned, and especially the assumption that a wet impervious soil is necessarily connected with a damp atmosphere; many "consumption-breeding places" can be found in England, where the soil is light and pervious, but where the presence of stagnant water, bad drainage, the want of a free circulation of air, from trees, buildings, and other obstacles, are, I believe, far more important causes than the character of the soil. That a soil impervious to a great extent to water exerts an injurious influence, there can, I think, be little doubt; but other important considerations must be taken into account before we can come to positive conclusions respecting the influence of soil, as shown by the examples I have given of Chelsea and Hampstead.

My space does not allow me to enter fully into the geological question on the present occasion.

Mr. Alfred Haviland ("Transactions," p. 145,) kindly made an estimate from the number of cases of tubercular disease in my last paper, and the geological character of the places where they occurred. His conclusions are—1 in 41, Old Red Sandstone; 1 in 39, London Clay; 1 in 38, Coal Measures; 1 in 36, Chalk; 1 in 34, New Red Sandstone Mr. Haviland remarks, "These figures are far from conclusive, and teach us little; but they point to a field that is still unexplored, and which, when once cultivated, will yield rich results."

TABLE.

The table shows the proportion of deaths from phthisis to the population, *during ten years*, from 1851 to 1860, in the 623 districts of England and Wales. Fractions are avoided as the data are necessarily imperfect. The results are obtained by dividing the population of each district by the number of deaths from Phthisis as reported in the Registrar General's return for 1864. Corrections in these returns, have been made by the Registrar-General, for hospitals, poorhouses, barracks, &c.

METROPOLIS. MIDDLESEX.

Kensington	35	St. Martin's-in-the-Fields.	28	Hampstead	65
Chelsea	28	St. James's, Westminster	34	Pancras	34
St. George's, Hanover Square	37	Marylebone	36	Islington	42
Westminster	28			Hackney	43
				St. Giles	26

METROPOLIS.

MIDDLESEX. *continued.*

Strand	33
Holborn	30
Clerkenwell	37
St. Luke	38
East London	35
West London	17
London City	46
Shoreditch	35
Bethnal Green	41
Whitechapel	27
St. George's-East	28
Stepney, Mile end Old Town	37
Poplar	37

SURREY.

St. Saviour	25
St. Olave	27
Bermondsey	41
St. George's	30
Newington	36
Lambeth	40
Wandsworth	41
Camberwell	41
Rotherhithe	47

KENT.

Greenwich	30
Lewisham	54

SURREY.

Epsom	54
Chertsey	56
Guildford	43
Farnham	49
Farnborough	53
Hambledon	47
Dorking	41
Reigate	44
Godstone	53
Croydon	49
Kingston	47
Richmond	47

KENT.

Bromley	49
Dartford	58
Gravesend	37
North Aylesford	52
Hoo	49
Medway	28
Malling	45
Sevenoaks	43
Tunbridge	37
Maidstone	35

KENT. *continued.*

Hollingbourn	44
Cranbrook	47
Tenterden	33
West Ashford	38
East Ashford	45
Bridge	50
Canterbury	36
Blean	49
Faversham	40
Milton	53
Sheppey	61
Thanet	36
Eastry	43
Dover	50
Eltham	53
Romney Marsh	48

SUSSEX.

Rye	37
Hastings	30
Battle	49
Eastbourne	43
Hailsham	40
Ticehurst	40
Uckfield	42
East Grinstead	46
Cuckfield	40
Lewis	31
Brighton	33
Steyning	49
Horsham	38
Petworth	35
Thakeham	35
Worthing	33
Westhampnett	37
Chichester	28
Midhurst	36
Westbourne	32

HAMPSHIRE.

Havant	39
Portsea Island	35
Alverstoke	19
Fareham	34
Isle of Wight	38
Lymington	37
Christchurch	31
Ringwood	43
Fordingbridge	37
New Forest	40
Southampton	30
South Stoneham	45
Romsey	40
Stockbridge	40
Winchester	34
Droxford	41

HAMPSHIRE. *continued.*

Catherington	40
Petersfield	44
Alresford	38
Alton	52
Hartley Wintney	37
Basingstoke	38
Whitchurch	34
Andover	38
Kingsclere	42

BERKSHIRE.

Newbury	38
Hungerford	42
Faringdon	32
Abingdon	34
Wantage	38
Wallingford	40
Bradfield	45
Reading	31
Wokingham	65
Cookham	48
Easthampstead	43
Windsor	34

MIDDLESEX.

Staines	51
Uxbridge	50
Brentford	42
Hendon	46
Barnet	33
Edmonton	51

HERTFORDSHIRE.

Ware	44
Bishop's Stortford	51
Royston	37
Hitchin	47
Hertford	38
Hatfield	54
St. Alban's	42
Watford	36
Hemel Hempstead	43
Berkhampstead	37

BUCKINGHAMSHIRE.

Amersham	40
Eton	47
Wycombe	43
Aylesbury	42
Winslow	38
Newport Pagnell	37
Buckingham	37

OXFORDSHIRE.

Henley	45

OXFORDSHIRE. *contd.*

Thame	35
Headington	30
Oxford	33
Bicester	33
Woodstock	36
Witney	41
Chipping Norton	43
Banbury	41

NORTHAMPTONSHIRE.

Brackley	48
Towcester	39
Potterspury	51
Hardingstone	35
Northampton	34
Daventry	48
Brixworth	48
Wellingborough	35
Kettering	55
Thrapston	46
Oundle	50
Peterborough	46

HUNTINGDONSHIRE.

Huntingdon	42
St. Ives	34
St. Neots	44

BEDFORDSHIRE.

Bedford	35
Biggleswade	46
Ampthill	32
Woburn	37
Leighton Buzzard	36
Luton	35

CAMBRIDGESHIRE.

Caxton	39
Chesterton	34
Cambridge	29
Linton	35
Newmarket	30
Ely	43
North Witchford	46
Whittlesea	41
Wisbeach	50

ESSEX.

West Ham	46
Epping	52
Ongar	81
Romford	46
Orsett	35
Billericay	41

ESSEX. *continued.*

Chelmsford	35
Rochford	45
Maldon	32
Tendring	39
Colchester	30
Lexden	40
Witham	33
Halstead	30
Braintree	26
Dunmow	33
Saffron Walden	35

SUFFOLK.

Risbridge	35
Sudbury	35
Cosford	40
Thingoe	33
Bury St. Edmunds	30
Mildenhall	28
Stow	34
Hartismere	38
Hoxne	30
Bosmere	41
Samford	34
Ipswich	33
Woodbridge	37
Plomesgate	29
Blything	39
Wangford	40
Mutford	46

NORFOLK.

Great Yarmouth	40
Flegg	39
Tunstead	36
Erpingham	33
Aylsham	39
St. Faith's	50
Norwich	37
Forehoe	48
Henstead	41
Blofield	43
Loddon	41
Depwade	36
Guiltcross	29
Wayland	36
Mitford	32
Walsingham	31
Docking	48
Freebridge Lynn	44
King's Lynn	47
Downham	44
Swaffham	47
Thetford	28

WILTSHIRE.

Highworth	37
Cricklade	45
Malmesbury	52
Chippenham	40
Calne	43
Marlborough	38
Devizes	34
Melksham	45
Bradford	39
Westbury	48
Warminster	48
Pusey	45
Amesbury	37
Alderbury	34
Salisbury	30
Wilton	37
Tisbury	53
Mere	41

DORSETSHIRE.

Shaftesbury	40
Sturminster	39
Blandford	45
Wimborne	39
Poole	45
Wareham	42
Weymouth	57
Dorchester	41
Sherborne	34
Beaminster	59
Bridport	49

DEVONSHIRE.

Axminster	46
Honiton	45
St. Thomas'	41
Exeter	31
Newton Abbot	37
Totnes	53
Kingsbridge	47
Plympton St. Mary	53
Plymouth	36
East Stonehouse	27
Stoke Damerel	42
Tavistock	44
Okehampton	40
Crediton	55
Tiverton and Dulverton	42
South Molton	47
Barnstaple	45
Torrington	43
Bideford	51
Holsworthy	45

CORNWALL.

Stratton	47
Camelford	47
Launceston	41
St. German's	42
Liskeard	41
Bodmin	40
St. Columb	44
St. Austell	40
Truro	38
Falmouth	43
Helston	29
Redruth	27
Penzance	33
Scilly Isles	35

SOMERSETSHIRE.

Williton	48
Wellington	45
Taunton	49
Bridgwater	41
Langport	53
Chard	44
Yeovil	42
Wincanton	42
Frome	46
Shepton Mallet	50
Wells	43
Axbridge	52
Clutton	51
Bath	37
Keynsham	40
Bedminster	50

GLOUCESTERSHIRE

Bristol	31
Clifton	39
Chipping Sodbury	46
Thornbury	47
Dursley	42
Westbury-on-Severn	58
Newent	48
Gloucester	37
Wheatenhurst	60
Stroud	44
Tetbury	43
Cirencester	43
Northleach	41
Stow-on-the-Wold	41
Winchcomb	45
Cheltenham	40
Tewkesbury	35

HEREFORDSHIRE.

Ledbury	44
Ross	44
Hereford	38

HEREFORDSHIRE. *continued.*

Weobly	50
Bromyard	57
Leominster	48

SHROPSHIRE.

Ludlow	56
Clun	39
Church Stretton	48
Cleobury Mortimer	45
Bridgnorth	46
Shiffnal	45
Madeley	40
Atcham	44
Shrewsbury	28
Oswestry	36
Ellesmere	43
Wem	37
Whitchurch	62
Market Drayton	39
Wellington	49
Newport	53

STAFFORDSHIRE.

Stafford	34
Stone	34
Newcastle-under-Lyme	35
Wolstanton	40
Stoke-upon-Trent	32
Leek	24
Cheadle	36
Uttoxeter	35
Burton-upon-Trent	41
Tamworth	43
Lichfield	48
Penkridge	43
Wolverhampton	41
Walsall	46
West Bromwich	55
Dudley	64

WORCESTERSHIRE.

Stourbridge	51
Kidderminster	37
Tenbury	63
Martley	52
Worcester	37
Upton-on-Severn	47
Evesham	42
Pershore	54
Droitwitch	51
Bromsgrove	42
King's Norton	58

WARWICKSHIRE.

Birmingham	36
Aston	47
Meriden	60
Atherstone	65
Nuneaton	43
Foleshill	39
Coventry	39
Rugby	48
Solihull	54
Warwick	37
Stratford-on-Avon	47
Shipston	46
Alcester	41
Southam	53

LEICESTERSHIRE.

Lutterworth	53
Market Harborough	45
Billesden	38
Blaby	45
Hinckley	38
Market Bosworth	40
Ashby-de-la-Zouch	47
Loughborough	34
Barrow-on-Soar	36
Leicester	33
Melton Mowbray	43

RUTLANDSHIRE.

Oakham	46
Uppingham	42

LINCOLNSHIRE.

Stamford	53
Bourn	58
Spalding	53
Holbeach	48
Boston	47
Sleaford	61
Grantham	50
Lincoln	47
Horncastle	59
Spilsby	37
Louth	37
Caistor	52
Glanford Brigg	42
Gainsborough	54

NOTTINGHAMSHIRE.

East Retford	51
Worksop	45
Mansfield	40
Basford	34
Radford	32
Nottingham	30

NOTTINGHAMSHIRE. *continued.*

Southwell	41
Newark	46
Bingham	40

DERBYSHIRE.

Shardlow	38
Derby	29
Belper	36
Ashbourne	37
Chesterfield	31
Bakewell	34
Chapel-en-le-Frith	40
Hayfield	26

CHESHIRE.

Stockport	31
Macclesfield	26
Altrincham	34
Runcorn	36
Northwich	38
Congleton	26
Nantwich	49
Great Boughton	33
Wirral	47

LANCASHIRE.

Liverpool	24
West Derby	32
Prescot	42
Ormskirk	42
Wigan	36
Warrington	38
Leigh	31
Bolton	31
Bury	32
Barton-on-Irwell	33
Chorlton	34
Salford	38
Manchester	26
Ashton-under-Lyne	27
Oldham	31
Rochdale	32
Burnley	32
Clitheroe	35
Blackburn	30
Chorley	33
Preston	27
Fylde	40
Garstang	31
Lancaster	31
Ulverstone	38

YORKSHIRE.

Sedbergh	33
Settle	35

YORKSHIRE. *continued.*

Skipton	30
Pateley Bridge	32
Ripon	37
Great Ouseburn	88
Knaresborough	35
Wetherby	89
Otley	32
Keighley	25
Todmorden	46
Saddleworth	26
Huddersfield	32
Halifax	32
Bradford	31
Hunslet	35
Leeds	36
Dewsbury	37
Wakefield	36
Pontefract	49
Hemsworth	48
Barnsley	39
Wortley	40
Ecclesall Bierlow	36
Sheffield	30
Rotherham	45
Doncaster	44
Thorne	42
Goole	46
Selby	59
Tadcaster	44
York	35
Pocklington	50
Howden	42
Beverley	48
Sculcoates	40
Hull	41
Patrington	41
Skirlaugh	53
Driffield	49
Bridlington	43
Scarborough	62
Malton	56
Easingwold	55
Thirsk	43
Helmsley	61
Pickering	52
Whitby	56
Guisborough	61
Stokesley	43
Northallerton	38
Bedale	56
Leyburn	55
Askrigg	43
Reeth	43
Richmond	45

DURHAM.

Darlington	34
Stockton and Hartlepool	45
Auckland	37
Teesdale	43
Weardale	33
Durham	45
Easington	67
Houghton-le-Spring	54
Chester-le-Street	46
Sunderland	46
South Shields	61
Gateshead	42

NORTHUMBERLAND.

Newcastle-on-Tyne	36
Tynemouth	45
Castle Ward	44
Hexham	40
Haltwhistle	35
Bellingham	44
Morpeth	48
Alnwick	45
Belford	64
Berwick-on-Tweed	49
Glendale	61
Rothbury	57

CUMBERLAND.

Alston	32
Penrith	39
Brampton	35
Longtown	42
Carlisle	32
Wigton	43
Cockermouth	48
Whitehaven	43
Bootle	37

WESTMORELAND.

Appleby, East Ward	43
Ditto, West Ward	35
Kendal	37

MONMOUTHSHIRE.

Chepstow	43
Monmouth	49
Abergavenny	30
Pontypool	39
Newport	37

GLAMORGANSHIRE.

Cardiff	34
Merthyr Tydfil	24

GLAMORGANSHIRE. *continued.*

Bridgend	42
Neath	33
Swansea & Gower.	34

CARMARTHEN.

Llanelly	31
Llandovery	23
Llandilofawr	28
Carmarthen	23

PEMBROKESHIRE.

Narberth	31
Pembroke	36
Haverfordwest	30

CARDIGANSHIRE.

Cardigan	28
Newcastle-Emlyn.	25
Lampeter	23
Aberayron	23

CARDIGANSHIRE. *continued.*

Aberystwith.	31
Tregaron	29

BRECKNOCKSHIRE.

Builth	43
Brecknock	38
Crickhowell	35
Hay	47

RADNORSHIRE.

Presteigne	46
Knighton	59
Rhayader	45

MONTGOMERY-SHIRE.

Machynlleth	25
Newtown	28
Montgomery	37
Llanfyllin	30

FLINTSHIRE.

Holywell	31

DENBIGHSHIRE.

Wrexham	37
Ruthin.	33
St. Asaph	32
Llanrwst	34

MERIONETHSHIRE.

Corwen	36
Bala	49
Dolgelly	36
Festiniog	40

CARNARVONSHIRE.

Pwllheli	41
Carnarvon	26
Bangor.	29
Conway	34

ANGLESEA.

Anglesea	27

GENERAL MORTALITY.

Of these 623 districts, the mean death-rate per 1000 during twenty years, 1841 to 1860, from all causes, varies from 15 to 34.

Glendale, Northumberland, 15; Bootle, Cumberland, 16; Easthampstead, 16; Eastbourne, 16; Bromley, 16; Steyning and Shoreham, 16; Cookham and Maidenhead, 16.

The districts that have the largest amount of mortality are Liverpool, 34 per 1000; Manchester, 32; Whitechapel, 29; St. George's in the East, 29; Leeds, 29; St. George's Southwark, 28; Bethnal Green, 28; East Stonehouse (including Naval hospital,) 28; Bristol, 28; Wolverhampton, 28; Sheffield, 28; Hull, 28; Wigan, 27; Bolton, 27; Oldham, 27; Newcastle-upon-Tyne, 27; Nottingham, 27; West London, 27; East London, 27; St. Luke's, 27; St. Giles, 27; Bermondsey, 27.

In Wales the lowest amount of general mortality is at Knighton, 17, and the highest at Merthyr Tydfil, 29.

I have quoted the above, so that the reader may make a comparison between the general death-rate and that from phthisis in the districts named.

DISTRICTS IN WHICH THE DEATHS FROM PHTHISIS ARE THE GREATEST AND THE LEAST.

The districts in which the smallest number of deaths occur from phthisis, in proportion to the population, are—

Between 80 and 90 — Wetherby, 89; Great Ouseburn, 88; Ongar, 81. Easington, 67.

Between 60 and 65—Wheatenhurst, 60; Meriden, 60; Hampstead, 61; Sheppey, 61; Sleaford, 61; Helmsley, 61; Guisboro', 61; Glendale, 61; Whitchurch, 62; Scarborough, 62; Tenbury, 63; Dudley, 64; Belford, 64; Atherstone, 65.

Between 55 and 60—Kettering, Crediton, Easingwold, Malton, Leyburn, Ludlow, Epsom, Weobly, Whitby, Bedale, Bromyard, Rothbury, Westbury, King's Norton, Bourn, Dartford, Epping, Weymouth, Beaminster, Knighton, Horncastle.

Between 50 and 55—Uxbridge, Battle, Bridge, Dover, Bishop's Stortford, Wisbeach, Oundle, St. Faith's, Shepton Mallet, Grantham, Pocklington, Eltham, Edmonton, Potterspury, Bridport, Clutton, Droitwich, Stourbridge, East Retford, North Alresford, Ampthill, Stoke Damerel, Tisbury, Malmesbury, Staines, Axbridge, Martley, Caistor, Pickering, Alton, Farnborough, Godstone, Milton, Totnes, Plympton, Longport, Bedminster, Newport (Salop), Southam, Lutterworth, Stamford, Spalding, Skirlaugh, Chertsey, Hatfield, Pershore, Gainsborough, Houghton-le-Spring, Solihull, Lewisham.

The above numbers, from 50 to 60, are placed as they come in succession, as regards the comparative frequency of phthisis to the population, the higher numbers indicating the greatest exemption. For places where the mortality is below 50, the tables must be consulted.

Let me now point out the districts in England and Wales where phthisis is most prevalent, where the proportionate number is 26 or below it. St. Giles, 26; West London, * 17; St. Saviour's, 25; Alverstoke (Hospitals), 19; Braintree, 26; Hayfield, 26; Macclesfield, 26; Congleton, 26; Liverpool, 24; Manchester, 26; Keighley, 25; Saddleworth, 25; Leek, 24; Newcastle Emlyn, 25; Merthyr Tydfil, 24; Llandovery, 23; Machynlleth, 25; Aberayron, 23; Carnarvon, 26; Lampeter, 23; Carmarthen, 23,

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* Western part of City, including St. Bartholomew's Hospital, but I learn at the Registrar General's Office, that corrections have been made for Hospitals, Barracks, Poor-houses, &c.

In the following summary the lowest and the highest rates of mortality from Phthisis are given in each county of England and Wales.

ENGLAND.

MIDDLESEX.
West London . 17
Kensington . . 35
Hampstead . . 61

SURREY.
St. Saviour's . 25
Godstone . . 53
Chertsey . . 54
Epsom . . . 56

KENT.
Medway . . 28
Canterbury . . 36
Sheppey . . 61

SUSSEX.
Chichester . . 28
Hastings . . 30
Steyning . . 49
Battle . . . 59

HAMPSHIRE.
Alverstoke . . 19
Southampton . 30
Christchurch . 31
Isle of Wight . 38
Alton . . . 52

BERKSHIRE.
Reading . . 31
Faringdon . . 32
Wokingham. . 65

HERTFORDSHIRE.
Watford . . 36
Hertford . . 38
Hatfield . . 54

BUCKINGHAM-SHIRE.
Newport Pagnell . 37
Buckingham. . 37
Eton . . . 47

OXFORDSHIRE.
Headington . . 30
Oxford. . . 33
Henley. . . 45

NORTHAMPTON-SHIRE.
Northampton . 34
Hardingstone . 35
Kettering . . 55

HUNTINGDON-SHIRE.
St. Ives . . 34
St. Neots . . 44

BEDFORDSHIRE.
Bedford . . 35
Biggleswade. . 46

CAMBRIDGESHIRE.
Cambridge . . 29
Newmarket . . 30
Wisbeach . . 50

ESSEX.
Braintree . . 26
Colchester . . 30
Epping . . 52
Ongar . . . 81

SUFFOLK.
Plomesgate . . 29
Mildenhall . . 28
Mutford & Lowestoft 46

NORFOLK.
Thetford . . 28
Guiltcross . . 29
St. Faith's . . 50

WILTSHIRE.
Salisbury . . 30
Devizes . . 34
Warminster . . 48
Tisbury . . 52

DORSETSHIRE.
Sherborne . . 34
Shaftesbury . . 40
Beaminster . . 59

DEVONSHIRE.
East Stonehouse . 27
Exeter . . . 31
Crediton . . 55

CORNWALL.
Redruth . . 27
Helston . . 29
Penzance . . 33
Camelford . . 47

SOMERSETSHIRE.
Bath . . . 37
Bridgwater . . 41
Axbridge . . 52
Langport . . 53

GLOUCESTERSHIRE
Bristol . . . 31
Tewkesbury . . 35
Gloucester . . 37
Wheatenhurst . 60

HEREFORDSHIRE.
Hereford . . 38
Weobly . . 50
Bromyard . . 57

SHROPSHIRE.
Shrewsbury . . 28
Ludlow . . 56
Whitchurch . . 62

STAFFORDSHIRE.
Leek . . . 24
Stafford . . 34
West Bromwich . 55
Dudley. . . 64

WORCESTERSHIRE.
Kidderminster . 37
King's Norton . 58
Tenbury . . 63

WARWICKSHIRE.
Birmingham. . 36
Meriden . . 60
Atherstone . . 65

LEICESTERSHIRE.
Leicester . . 33
Ashby-de-la-Zouch 47
Lutterworth . . 53

RUTLANDSHIRE.
Uppingham . . 42
Oakham . . 46

LINCOLNSHIRE.

Louth	37
Spilsby	37
Horncastle	59
Sleaford	61

NOTTINGHAMSHIRE.

Nottingham	30
Radford	32
Basford	34
East Retford	51

DERBYSHIRE.

Hayfield	26
Derby	29
Chapel-en-le-Frith	40

CHESHIRE.

Congleton	26
Macclesfield	26
Wirral	47
Nantwich	49

LANCASHIRE.

Liverpool	24
Manchester	26
Ormskirk	42
Prescot.	42

YORKSHIRE.

Keighley	25
Saddleworth.	26
Hemsley	61
Scarborough.	62
Great Ouseburn	88
Wetherby	89

DURHAM.

Weardale	33
Easington	67

NORTHUMBERLAND.

Haltwhistle	35
Glendale	61
Belford.	64

CUMBERLAND.

Carlisle	32
Alston	32
Cockermouth	48

WESTMORELAND.

West Ward	35
Kendal	35
East Ward	43

WALES.

MONMOUTHSHIRE.

Abergavenny	30
Newport	37
Monmouth	49

GLAMORGANSHIRE.

Merthyr Tydfil	24
Neath	33
Swansea	34
Bridgend	42

CARMARTHEN.

Llandovery	23
Carmarthen	23
Llanelly	31

PEMBROKESHIRE.

Haverfordwest	30
Pembroke	36

CARDIGANSHIRE.

Lampeter	23
Aberayron	23

BRECKNOCKSHIRE.

Crickhowell	35
Builth	43
Hay	47

RADNORSHIRE.

Presteigne	46
Knighton	59

MONTGOMERYSHIRE.

Machynlleth.	25
Montgomery.	37

FLINTSHIRE.

Holywell	31

DENBIGHSHIRE.

St. Asaph	32
Wrexham	37

MERIONETHSHIRE.

Corwen	36
Bala	49

CARNARVONSHIRE.

Carnarvon	26
Bangor.	29
Pwllheli	41

ANGLESEA.

Anglesea	27

Taking the Coast-line of England, commencing on the extreme North East, Berwick-on-Tweed, and ending with New Romney on the extreme South East, the following is the rate of mortality on the Eastern coast:—Berwick-upon-Tweed, 49; Belford, 64; Alnwick, 45; Morpeth, 48; South Shields, 49; Sunderland, 46; Houghton-le-Spring, 54; Hartlepool, 45; Guisborough, 61; Whitby, 56; Scarborough, 62; Bridlington, 43; Partington, 41;

Spilsby, 37; Holbeach, 48; Yarmouth, 40; Lowestoft, 46; Aldborough and Plomesgate, 29; Gravesend, 37; Isle of Sheppey, 61; Isle of Thanet, 36; Folkestone, 50; New Romney, 48.

On the South coast, beginning with Hastings, and ending with Penzance:—Hastings, 30; Brighton, 33; Isle of Wight, 38; Christchurch, 31; Weymouth, 57; Race, 35; Bridport, 49; Lyme Regis, 45; Sidmouth, 45; Teignmouth, 37; Exmouth, 41; Dartmouth, 53; Helston, 29; Penzance, 33.

On the West coast, beginning with St. Ives, and ending with Carlisle:—St. Ives, 33; Bristol, 31; Clifton, 39; Newport, 43; Bridgend, 42; Swansea Bay, 33; Tenby, 41; Pembroke, 41; Narberth, 31; Cardigan, 28; Conway, 27; Holywell, 31; Liverpool, 24; Cockermouth, 48; Whitehaven, 43; Carlisle, 32.

The subjoined figures give the proportion of deaths from phthisis in the fifty-two capital towns of England and Wales. Those having the greatest mortality are placed first. Liverpool, 24; Manchester, 26; Chichester, 28; Cambridge, 29; Shrewsbury, 29; Derby, 29; Nottingham, 30; Salisbury, 30; Lancaster, 31; Reading, 31; Exeter, 31; Carlisle, 32; Leicester, 33; Oxford, 33; Ipswich, 33; York, 35; Winchester, 34; Nottingham, 30; Stafford, 34; Bedford, 35; Chelmsford, 35; Maidstone, 35; Newcastle, 35; Norwich, 37; Buckingham, 37; Gloucester, 37; Warwick, 37; Hereford, 38; Isle of Wight, (including Ryde, Newport, Cowes, and Ventnor) 38; Worcester, 37; Dorchester, 41; Bodmin, 40; Huntingdon, 42; Guildford, 43; Durham, 45; Appleby, 43; Oakham, 45; London, City, 46; Lincoln, 47; Taunton, 49; Monmouth, 49.

WALES.

Carmarthen, 23; Carnarvon, 26; Anglesea, 27; Cardigan, 28; Pembroke, 36; Flint, including Holywell, 31; Cardiff, 34; Dolgelly, 36; Wrexham, 37; Brecknock, 38; Radnor, including Presteigne, 46.

If space would permit, I could readily show that the salubrity of many of these cities and towns might be greatly improved by better drainage, by a freer circulation of air, and by the removal of many contaminating influences that now exist. One fertile cause of phthisis, I believe, in cities and towns, is the residence of a great number of people in damp kitchens, and rooms below the ground level; and this may to some extent account for the great

prevalence of the disease among domestic servants; but this is a question I shall have to consider in a future paper.

WALES.

This country demands special notice, because, as will be seen by the table, phthisis is here very prevalent; and yet many have supposed that its generally moist and warm atmosphere would, to some extent, prevent the occurrence of the disease. There is one remarkable circumstance connected with the Welsh returns that is of great practical importance, viz., that whilst the death-rate from phthisis is high, the *general* mortality, with the exception of Merthyr Tydfil (29 per 1000); Cardiff, 23; Crickhowell, 26; Wrexham, 22; Neath, 22; Brecknock, 21; Newtown, 21; Holywell, 21; Carnarvon, 21; Bangor, 21; Carmarthen, 21. The general mortality in the other unions ranges from 20 to 18 per 1000, in 20 years, 1841 to 1860. In Builth and Knighton it is as low as 17 per 1000; and in these districts the mortality from phthisis is only 43 and 59; the proportion of square acres to each person being 12·37 and 9·82; and I may here remark that the acreage to the population is far greater in Wales than in England, the lowest being Merthyr Tydfil, 1 acre ·23; the highest, Rhayader 15·51. It is also important to mention that, although in Wales as in England, the female mortality from phthisis is greater than that of the male, in eleven unions, Merthyr Tydfil, Llandovery, Carmarthen, Cardigan, Newcastle-Emlyn, Lampeter, Tregaron, Montgomery, Holywell, Corwen and Conway, the male mortality from phthisis exceeds that of the female.

REGISTRATION OF DEATHS FROM PHTHISIS.

Several of my correspondents believe that many cases are registered under the head of Pulmonary Consumption that do not belong to this disease, and there is probably much truth in their statements; but, on the other hand, as I said in my last paper, *a great number of deaths that would come under the division of lung-tubercle are not included in this list.* I have recently attended two cases of chronic fibrous phthisis, neither of which probably were properly registered. The last was a young lady in the Brompton Road, who for many years had laboured under chronic phthisis, as I ascertained by a post-mortem examination. The upper parts of

both lungs were firmly bound to the chest walls. The lung structure dense, solid, with a large amount of fibrous tissue, and several small cavities. About one-fourth of the lung structure only was in a normal condition. A sudden attack of sore throat and bronchitis, as might have been expected, quickly terminated the life of this patient. I registered the death as follows:—Primary disease, phthisis, secondary *bronchitis*. Probably this death was registered as one of *bronchitis*, although the alteration of lung-tissue from the previous disease was indirectly the cause of the fatal termination. I mention this case for the purpose of showing the great difficulty in some instances, even where the body has been examined after death, of obtaining an exact registration, and I could greatly multiply the examples. These remarks, I scarcely need say, are equally applicable to many other diseases.

It is probable too, in some districts, where, in certain occupations, extraneous bodies are received into the lungs, as needle pointers, file cutters and grinders, stone masons, colliers, cloth dressers, feather sorters, workers in flax, and other dusty occupations, that bronchitis, miner's asthma, lung consolidation, and some other lesions, are often registered under the head of phthisis. In many of the above-named diseases there is a deposition of tubercular matter around the extraneous body, which may soften and form a cavity, but in many other examples there is no true tubercular deposit.

CAUSES OF PULMONARY CONSUMPTION.

Although I believe that a damp atmosphere has much influence in the production of this disease, other potent causes, as is well known, are in operation, which it will be necessary to consider more fully than I have hitherto done, for the purpose of arriving at a right understanding of this question. In many districts where the soil and atmosphere are generally dry, local causes may be in existence that serve to swell the mortality. On the other hand, in some localities, less favourably placed, the inhabitants, from the more healthy nature of their occupations, and the greater sparseness of the population, may be more exempt from the disease than those first mentioned. As I have said before, phthisis may be prevalent in one parish or district, and in another, not very far off, it may be comparatively rare. I could adduce a vast number of examples of this, but those already mentioned of Chelsea and Hampstead will

suffice. I have not space to enter into the geological question at the present time, but I believe, as I have mentioned before, that the nature of the soil has less to do with the production of phthisis, than the height above the sea level, and *the free circulation of air*. That soil impervious to water is likely in a flat country to produce dampness of atmosphere is tolerably certain ; but on many sandy and gravelly soils phthisis is fearfully prevalent, and on the contrary persons living on a clayey soil may be comparatively exempt from the disease. The table affords abundant evidence of this.

There are several important matters to bear in mind in considering these estimates, viz., that a large portion of the surrounding district, as well as that of the places mentioned, is often included, so that, as in the example of Lambeth, where Sydenham and Norwood form a part of the area, one place may be comparatively free from phthisis, whilst in another the mortality from this cause may be in excess. Again, the presence of barracks or hospitals may serve to swell the mortality. In some watering places, too, as Hastings, the Isle of Wight, Brighton, Weymouth, Sidmouth, Penzance, Swansea, Clifton, and others, the death-rate from the presence of visitors may be greatly increased.

Dr. G. Moore, of Hastings, informs me that the death-rate from phthisis at that place "is greatly increased by the presence of visitors;" but I think, as a rule, patients labouring under this disease do not generally die from their homes.

Another circumstance worthy of note is that families predisposed to phthisis may settle in districts that are supposed to be favourable to consumptive patients, and hence, after a time, the mortality will be increased from such removals. Many persons, I believe, have been sent to Chelsea, under the erroneous impression that its comparatively mild, warm atmosphere was favourable to those predisposed to phthisis. The contiguity of the Brompton hospital for consumption has tended also to strengthen this belief.

SEX.

As I have before stated, females are more liable to phthisis than males. Thus, in the ten years named, of the 508,923 deaths from phthisis, the females exceeded the males by 30,313, but on looking over the table of the 623 districts, I find that there are several examples where the male deaths are greatly in excess. Thus, at Alverstoke, including Gosport and the Haslar Hospital, the

male population is 10,833, and the deaths of males from phthisis, 783 ; the female population, 8948, and the deaths from phthisis only 226 ; but the presence of the hospitals named, and the employment of young men in an unhealthy locality, as shown in the letter of Dr. MacCormack, are sufficient to explain this difference. At Westminster, although the female population is greater than that of the male, the male deaths are 1350, the female deaths 988. As mentioned before, in eleven of the Welsh unions the male deaths are in excess.

At Bedford, the male population is 17,835, the female 18,962 ; the deaths from phthisis among the former 382, among the latter 607. At Ampthill in the same county, and at Leighton Buzzard, the female mortality is also greatly in excess. At Belper, the males are more numerous, yet 821 females die of phthisis, and only 514 males. At Chesterfield the proportion of females is as great as it is also at Congleton. At Newmarket the number of males exceeds that of the females by 103 ; but the deaths of the latter from phthisis are 532, of the former 346. In Tendring the males exceed the females by 353, but the deaths among the former are 442, among the latter 259. At Lexden, Colchester, the female deaths are nearly double those of the males. At Halstead the excess of female deaths is also very great. At Braintree a third more. In the Plomesgate Union, Thetford, Wimborne, Dorchester, Tavistock, Cirencester, Madeley, Newcastle-under-Lyme, Wolstanton, Leek, Ashby-de-la-Zouche, Barrow-on-Soar, Melton Mowbray, Spilsby, Glanford Brigg, Bath, Birmingham, Sheffield, Worthing, Ecclesall Bierlow, Llandilofawr, Carmarthen, Haverfordwest, Cardigan, Newcastle-in-Emlyn, Lampeter, Aberayron, Tregaron, Holywell, Bala, Dolgelly, Conway, Gainsborough, Mansfield, Basford, Shardlow, Derby, Belper, Bakewell, Hayfield, Stockport, Macclesfield, Leigh, Bolton, Ashton-under-Lyne, Haslingden, Clitheroe, Blackburn, Lancaster, Skipton, Halifax, Dewsbury, Wakefield, Wigan, and other places, the deaths of females are greatly in excess.

At Bury, Lancashire, the sexes are about equal, but the deaths of females amount to 1707, the males to 1280. At Burnley, the female population is only about 1000 more than the male, but the female deaths are 388 more than those of the males. At Keighley the excess of female deaths is nearly as great. At Saddleworth the proportion of female mortality is even greater. At Bradford it is 800 more than that of the males. At Carnarvon the female deaths exceed those of the males by 184.

I have devoted a large space to this question, because it is one of great importance, the occupation and overcrowding of the females having evidently more to do with the mortality than soil or a damp atmosphere. In my last paper I stated that the exposed state of many of our railway stations and steam-boat piers was a fertile exciting cause of pulmonary consumption; persons predisposed to phthisis often get heated from over exertion; and as some of these unprotected places are exposed to a cutting wind, local inflammation of the lung or lungs follows, and next succeed tubercular deposit. At most of the places enumerated above, the causes of the great excess of female mortality can be made out with tolerable clearness, and to some extent are remediable. Crowded rooms, a vitiated atmosphere, long continuance at work at an early age, and the situation of the building or manufactory, overcrowding in cottages and in apartments, are all matters with those above enumerated that demand the attention of the government. If a sanitary inspector were appointed for every county, and if he were obliged to make an annual report to the government on the causes in his locality that affect human longevity, as well as the supply of food to the people, including the prevailing diseases among plants and our domestic animals, a great saving of life, I believe, would be effected.

FIELD LABOUR.

In many districts in England it has been the custom to work young girls and women in gangs in the fields, but I have reason to believe that, although these persons are often exposed to the inclemency of the weather, the occupation is far more healthy than that of close confinement to needlework, the congregation of a number of persons in a manufactory, or other in-door employments. I speak partly from inquiries that I have made myself, and partly from the evidence afforded by correspondents. In the Tendring Union, where girls have not been worked in gangs, and not much employed in field labour, the female mortality from phthisis is greatly in excess of the male. At Great Ouseburn, one of the healthiest districts in England, girls and women have often been so employed, and so in many agricultural districts, where the mortality from phthisis is generally far less than in towns. One of my correspondents in the Tendring Union, Mr. Stanford, believes that if girls in this district were more occupied in field work,

the mortality from phthisis would be greatly reduced, and I think that he is right, the more fitting and womanly occupation being not always the most healthy.

UNDERGROUND KITCHENS AND DAMP DWELLINGS.

I have already spoken of the great prevalence of phthisis among female servants, and that their residing and sleeping in damp underground kitchens in cities and towns is not an unfrequent cause of lung tubercle, but this does not apply to servants only. Many persons of a better class, including children, in the suburbs of London and other large towns and cities, let all the upper part of the house, and live chiefly below the ground level. Houses, too, are now built and inhabited so quickly that it is utterly impossible that they can be free from damp and fit for human habitations. This, too, may be another cause of the increase of this fatal disease. I am not aware that these causes have been specially alluded to by any previous writer.

MINES.

The presence of mines in a district is also another fertile source of tubercle, as shown by several of my correspondents. A damp atmosphere, insufficient and innutritious food, early and imprudent marriages, and often the ascent of ladders after a hard day's work, all serve to swell the tables of mortality. Those who have visited some of the Cornish and Welsh mines will not be surprised at the prevalence of phthisis among the miners. The great mortality in some of the Welsh districts is, I suspect, greatly attributable to this cause. In some mining districts, however, the mortality is considerably less, much depending probably on the dry or wet state of the mine and the material worked; but this is a question that yet requires careful investigation.

DENSITY OF POPULATION.

As I have said before, as a general rule, the prevalence of phthisis corresponds to a great extent with the density of the population, and to the general amount of mortality; but to this rule there are many exceptions—exceptions that can only be explained by the influence of deleterious causes, such as a damp

atmosphere, and the occupations and habits of the people. In nearly all the above mentioned towns where the mortality is excessive, the population is dense, the number of acres to each person varying from ·01 to ·47, whereas in the healthy districts mentioned above the number of acres to each person generally varies from 2·10 to 3·99.

But let me now point out some of the remarkable exceptions I have alluded to for the purpose of showing that other causes besides density of population have a potent influence. At Braintree, Essex, the mean rate of mortality for twenty years is 21 per 1000, and the square acres to each person 2·23 ; but the deaths from phthisis in ten years are 1 in 26. At Ongar, in the same county, 1 in 81, the general death-rate being 18 per 1000, and the acres to each person 4·10. At Thetford, Norfolk, including Brandon, the mean rate of mortality for twenty years is 20 per 1000, the acres to each person 6·44, and the mortality from phthisis 28. At Norwich the general mortality is 25 per 1000, the number of persons to the acre ·06, but the mortality from phthisis only 1 in 37. In the Plomesgate Union, Suffolk (29), the general mortality is 20 per 1000, the acres to each person 3·56. At Ipswich the general mortality is 22 per 1000, the acres to each person ·24, and the mortality from phthisis 1 in 33. At Chichester, including Bognor, where the mortality from phthisis is as low as 28, the number of persons to the square acre is only 1·59, and so with a great many other places I could name, for the purpose of showing that the presence of phthisis is not so much caused by density of population as many have supposed. In Wales, evidence of this is far more abundant than in England.

ILL ASSORTED MARRIAGES.

There can be but little doubt that ill-assorted marriages amongst all ranks tend much to increase the spread of this disease. In country districts especially, persons living in the same locality, and often in an unhealthy neighbourhood, form matrimonial alliances when both have an hereditary predisposition to pulmonary consumption. Cousins and second cousins frequently marry, both having an hereditary tuberculous taint. If, however, these persons selected, when practicable, healthy companions for life, and healthy districts, there would be a good chance of preserving their offspring from this fearful disease. The question is one beset with innu-

merable difficulties; but it is one that should receive more consideration than is given to it at present. Much may be done by a better supervision of our factories, mines, barracks, workshops, workhouses, dwellings of the poor, steam-boat and railway piers, town drainage, labour of the young, and of all occupations and employments that are detrimental to health; but if persons predisposed to phthisis continue to marry, irrespective of the fearful consequences they entail upon their offspring, there is but little chance of diminishing the death-rate. Many of my correspondents speak of these intermarriages as the cause of the greater prevalence of the disease in certain districts. Mr. J. H. Harris, surgeon, of Mildenhall, Suffolk, in speaking of Lakenheath, a village of 1800 inhabitants, says, "I attribute the great prevalence of phthisis in this and other places named mainly to close intermarriages." Mr. Amyot, surgeon, of Diss, believes that the great mortality from phthisis, in some parts of Depwade, arises from intermarriages and the bad construction of the dwellings of the poor. Dr. Williams, of Menai Bridge, Dr. Sheen, of Cardiff, Mr. Harris, of Redruth, and several others, especially mention the injurious effect produced by intermarriage.

INTERMITTENT FEVER AND PHTHISIS.

As regards the exemption from phthisis of the inhabitants of marshy districts where intermittent fever is prevalent, I believe that the opinion rests upon no solid foundation. I know of some aguish districts in England where phthisis is very common, and along the western coast of Africa, where this disease is so fatal to our sailors, the malarious poison, by depressing the vital powers, often lays the foundation for tubercular deposit, or hastens the fatal termination when the disease is established. Several of my correspondents speak of the diminution of phthisical cases, since the land has been better drained, and since ague has been less prevalent. I know of several districts in England where ague forty years since was very prevalent, but where, at the present time, its occurrence is rare and unfrequent.

As mentioned by Hérard and Cornil, "De la Phthisie Pulmonaire," 1867, pp. 637-640, the evidence upon this subject is very contradictory, as shown by many authors quoted. They observe, "Nous ne serions pas eloignés de croire que le climat chaud humide, et à temperature uniforme de la plupart des contrées marécageuses

constitue une condition jusqu'a un certain point favourable aux tuberculeux et s'oppose au développement de la phthisis."

I believe that this opinion is entirely erroneous. Bricheteau and many others have shown that cold moist climates like Strasburg are ravaged with phthisis and intermittent fevers. And so of many other districts and countries that might be named. I have introduced this subject, because it has an important bearing upon the influence of a damp atmosphere in the production of phthisis. I believe the error has arisen in consequence of the imperfect nature of the statistics formerly obtained.

ANSWERS FROM CORRESPONDENTS.

When I have found a great disparity as to the number of deaths from phthisis in any county or locality, I have generally written to medical practitioners in these districts to endeavour to ascertain the cause or causes of the high or low amount of death-rate from phthisis. The correspondence is too voluminous to publish in full, but its condensation will form one of the most important additions to this essay. It will especially serve to show that a vast amount of careful investigation, a great deal of sifting and winnowing of the evidence, is necessary before correct conclusions can be arrived at; geological formation, state of the drainage, height above the sea level, prevailing winds, nature of the food and occupations of the people, the free circulation of air, intermarriage among relatives, and among persons in the same unhealthy district; early employment of the young, especially of females, in manufactories and in sedentary occupations; the injurious effect of certain trades and employments by which extraneous substances are introduced into the air-tubes, the state of and situation of the habitations of the poor, and other causes, must be taken into account when our estimates are made.

Dr. Cooper Rose, of Hampstead, says, "The elevation of this place, 420 feet above the level of the Thames, keeps us above the basin of moisture, into which we very perceptibly descend below the point of Haverstock Hill. I frequently leave my house in clearness and sunshine, and find myself enveloped in moisture and fog after going down the hill to the point spoken of. The soil is mostly clay, with here and there a mound of gravel and sand. The heath, where there are but few houses, is nearly all sand. The drainage is now tolerably perfect, but this has been only

during the last six or eight years." I may add, when these returns were completed.

Mr. Harrison, surgeon, of Braintree, Essex, "attributes the difference in mortality from phthisis at this place (26) and at Ongar (81) to the circumstance that Ongar is purely agricultural, and that at Braintree two-thirds of the population are mill hands. The town is well drained, the subsoil various, in parts gravel, in parts clay, the rain-fall generally less than in other counties."

Mr. Potter, surgeon, of Ongar, Essex, fully bears out the statements of Mr. Harrison, and "believes that the manufacture of crape accounts to some extent for the excessive mortality from phthisis at Braintree." He also remarks of Ongar, that "although the people are not over well fed, this is one of the healthiest parts of the kingdom."

Dr. Bree, of Colchester (30), believes that intemperance, smoking and drinking at an early age, close dwellings, and certain trades and manufactures, all add to the mortality from this disease. Colchester, he adds, "stands on a hill, and the atmosphere is dry and bracing, but in spring a succession of dry cutting east winds very soon pick out the phthisical."

Mr. Amyot, of Diss, Norfolk, says, "Our hundred, Depwade and Diss, stands rather badly on your list, and its physical condition would bear out the inference, for most of its parishes have a deep stiff clay and brick earth beneath the vegetable soil, and consequently a heavy, moist atmosphere. But the Guiltcross district (29) should, on the same grounds, stand well, for its villages are mostly well placed in an extensive and open chalk district, with dry invigorating air and but little standing water."

Mr. R. Thompson, surgeon, Brandon, Suffolk (Thetford and Brandon) (28) believes that phthisis is much less prevalent of late years. Formerly the manufactory of gun flints was carried on to a large extent. Mr. Thompson believes "that the production of phthisis was due more to the exposure to heat, and the exhalations of moisture from the ground floors, than to the particles of flint that might enter the air passages. The soil is generally sand, and in some places chalk." Mr. Thompson speaks of Brandon, not of the other part of the union, Thetford.

Dr. M. S. Mac Cormack, the medical officer of health, Southampton, has afforded me some valuable information, which I regret that I am compelled from want of space to abbreviate. I wrote to my friend Dr. Wiblin respecting the cause of the great mortality

from phthisis at Southampton (30) and Alverstoke (19), and he kindly referred me to Dr. Mac Cormack. In Southampton, Dr. Mac Cormack states "that although the town has been thoroughly drained for nearly twenty years, the death-rate from phthisis has gradually increased until it now amounts to five and a half of all the deaths. Dr. Mac Cormack has been desirous of making a careful examination of the death registers, but the authorities give him no encouragement. He believes that the greater part of the mortality from phthisis would be found to be in low-lying districts of the town. In the valley of the Itchen, which includes a large low-lying marshy district, the mortality from this disease is also very great."

"The great fatality at Alverstoke among the male population arises partly from the number of marines and other military men. The men in the forts are in casemates, which are damp, dark, and badly ventilated. Many of the forts are surrounded by a wet ditch; besides these causes, many of the houses at Alverstoke are built along the shore of a muddy creek, many of them below low-water mark and always damp. As regards the barracks Fortori a more unhealthy locality could scarcely have been found. A large seafaring population, composed of men who have been in various climates, many of whom come home invalided, and the large Naval Hospital of Haslar, also serve to swell the death-rate."

Dr. E. Chinnery, of Lymington, Hants, says, "Alverstoke (19), owing to its proximity to the Solent, and to a number of water meadows, is damp and moist. A large number of men are employed on the government works, which accounts for the excessive mortality among the males. Alton (52) has very little water in its neighbourhood, and the air is cold, dry, and bracing.

Dr. Cockcroft, of Keighley, Yorkshire, where the mortality from phthisis is the greatest in that county (25), and where the female deaths exceed those of the male by 280, says, "The careless habits of the people, who often go in damp clothes to work in very warm crowded mills, and the sending children to work at an early age to some workshops, not under the Factories Act, may, in some measure, account for the great mortality; the land generally is well drained, but there are many floods in the valley."

Great Ouseburn, in the West Riding of Yorkshire, is one of the places in England where phthisis is least prevalent, the general mortality is only 18 per 1000, and the number of persons to the square acre 4·37. Mr. Staniland, of Bow, North Devon, who formerly resided there, speaks of Great Ouseburn as "one of the

healthiest villages in England; well drained, high and dry, on sandstone with a gravel subsoil; but some parts of the union are low and damp."

Dr. Leonard Sedgwick, of Gloucester Terrace, who is well acquainted with this district, says, "The greater part of the union is well drained, well cultivated and open, on gravel and red sandstone; some is flat, and subject to annual inundations; there is one large tract of stiff clay on a low level, and some smaller ones. The labourers are agricultural, the women are much employed in out-door work, and wages are good. It is thinly wooded as a rule; but, as it is some twenty miles long, by five or six, or more, wide, a general description is not applicable to all its parts. The largest place in it, Boroughbridge, with 960 inhabitants, is much of it low, and, in consequence of frequent floods, damp rather than otherwise."

Dr. Buckle, of Chichester, where the mortality from phthisis is excessive (28), tells me "That this town is situated on a nearly level tract of land. Its climate is mild, and often damp and depressing; its natural drainage is sluggish, and the artificial drainage is into cesspools dug in the gravelly soil, from which we procure our water by digging wells often very close to our cesspools. We are subject, at times, to the soil becoming waterlogged, the water rising and entering our cellars and kitchens. Our water, as you may suppose, is not very pure. Endeavours have been made to induce our Town Council to drain the town, but without avail, and even some of our medical brethren are against it. They seem to prefer paying a death toll to a drainage rate! You will gather enough from this short account of Chichester to help you to classify the place in relation to dampness of soil and phthisis."

Dr. H. Day, of Stafford, in answer to the question, why is there so much difference in the mortality from phthisis in Leek (24), Stafford (34), and Dudley (64), remarks, "Leek is close to and almost surrounded by a cold, barren, bleak country, with a damp soil. The registration is very imperfect, quacks and druggists are most improperly allowed to give certificates, bronchitis and asthma being not unfrequently registered under the head of phthisis. The silk mills at Leek, where persons are employed often in hot, damp, close rooms, with gas burnt for long periods, serve also to explain the great amount of mortality; added to these, the intemperance and bad living of the weavers generally, and the extending hereditary predisposition, must be taken into account."

"Dudley is a large mining district, parts of the surrounding country being hilly, and the soil generally light and dry. The population is engaged in mining and in iron work, no close workshops, but labour more resembling out-door work. Besides, Dudley obtains numerous recruits from the surrounding open country and from Wales; Leek derives its workers from an old stock, propagating degeneracy; hence the difference may, to a great extent, be accounted for."

Dr. Cordwent, of Taunton, says, "Perhaps, the high rate of death from phthisis recorded at Bath (37) is due to the fact that it is the sanatorium of the west of England, and thus it has a non-indigenous mortality. Taunton (49) is on a considerable elevation in the valley. Bridgewater (41) is nearly on the level and in the immediate neighbourhood of the marsh, of which from 10 to 15000 acres are covered with water twice a year. From an eminence you may see daily 'the fogs rising,' as the people say, but in reality, condensing. Yeovil (42) may be slightly above the level of Bridgewater, and is built on the borders of what must have been a creek, in the pre-historic coast line. Langport (53), Axbridge (52), are on the plateau beyond the geological coast line. The soil is, I believe, clay over blue lias."

In Lincolnshire, the mortality from phthisis is comparatively small, for although in many parts there is a large quantity of water, it is not stagnant; the county is well drained, and there is a free circulation of air. Dr. J. Bourne, of North Somercotes, Louth, where the mortality from phthisis is the greatest, and this is not lower than 37, thinks "that the bad drainage of the lower part of the town accounts, to some extent, for the increased mortality. The atmosphere is on the whole bracing, and not generally foggy. Owing to the improved state of the drainage ague is now but rarely met with in this county."

Dr. Jacobson, of Sleaford (61), Lincolnshire, says, "Of 539 deaths in my practice, 51 were from phthisis, one in $10\frac{29}{51}$; the greater number of these were among the poorer classes. The air here is for the most part moist and relaxing, and persons coming from other parts often get quickly stout. Drainage is very good, and the lower orders have good food and wages. Ague is very rare."

Dr. Wrangham, of Wragby, Lincolnshire, in the immediate neighbourhood of the Fens, says, that he is "unable to account for the increased mortality of Louth, situated at the foot of the Wold

Hills, these hills being chiefly lime stone. Under the improved system of drainage, water never remains long on the surface. During my pupilage, thirty years since, ague was the prevailing disease; now it is very rare. The atmosphere here has been very damp and foggy for the last fortnight (December), and is so generally during the rainy season."

Dr. Boulton, of Horncastle, in addition to some very valuable information, has kindly forwarded to me a map of the geological formation of the county. Dr. Boulton says, " Pray do not suppose, that the land of Lincolnshire is boggy, foggy, or marshy. The drainage of the fens commenced under the direction of Sir Joseph Banks, eighty years since; the main drains are in fact navigable canals. The land is perfectly dry, and is now one of the finest corn growing districts of the kingdom. The marsh now consists chiefly of fine rich pasture land, perfectly dry, though divided by water ditches. November fogs are rare. Taking the places mentioned Louth (37), Sleaford (61), Spilsby (37), Horncastle (59), the people are placed much under similar circumstances; they are chiefly engaged in agriculture, and the trades dependent upon it; they are well fed, well shod, well clad, and well paid. The towns are all upon the chalk or near to it. Ague is scarcely ever met with, even in the fen; but to show that there is still some malarious influence, disorders are apt to take an intermittent type. My own personal experience of Lincolnshire is this, that when I came into the county in 1827, ague, typhus, and dysentery were very common. Ague reached from the Fens on to the Wolds; almost every disease took an intermittent form, and acute diseases in those days required the lancet, and were so cured. There was a traditional belief amongst fen-men of that day, and medical men of the fen country shared in it, that if they had the ague, they enjoyed a singular immunity from phthisis: but this, of course, was based upon no statistical evidence. My own impression is, and other medical men in this neighbourhood are of the same opinion, that phthisis among the young is much less frequent than in the early part of my career, better air, houses, food, and clothing having much to do with the improvement." I regret that want of space prevents my adding other interesting information. I have purposely devoted a larger space to this county, because its general salubrity stands very high, as well as its exemption from phthisis.

In Cambridgeshire the smallest fatality from phthisis is at Wisbeach, although the general mortality for twenty years is as

high as 23 per 1000 ; whereas, at Cambridge, the mortality from phthisis is 29, and the general mortality only 21 per 1000. The number of students here will scarcely affect the mortality from consumption, as but few, if any, students die of this disease at the University. Dr. Bury, at Wisbeach, is "surprised at the low mortality from phthisis in this town, as the hygienic condition is bad ; part of the town is imperfectly drained into the tidal river, and the most populous part is without any system of sewerage. Until recently cesspools were side by side with the drinking wells. It can no longer be said that we are surrounded by fen or bog, as this neighbourhood is as well drained as any in England. Still ague is among us, but in a mild and modified form, attacking generally the young and enfeebled; phthisical subjects suffer sooner from it." I may remark, in addition to Dr. Bury's evidence, that this union includes other parishes, with a total population of 34,769, so that the town alone must not be looked to.

The Mutford Union (46), Suffolk, is the most exempt in this county from phthisis. Dr. Peskett, of Beccles, Wangford district (40), says, "The Waveney marshes, generally drained, although some of them are undrained and subject to inundations, are in some places a mile in width. The soil for the most part is mixed—the population generally agricultural. We have had no ague since the 'comet year,' about ten years since, and then it was chiefly on the borders of the marshes. North-east winds are prevalent, and pleurisy, pneumonia, and bronchitis are common."

The comparative immunity of this district arises probably, as in Lincolnshire, from the open character of the country, and from the free circulation of air.

At Nantwich, Cheshire, including Crue, where the mortality is only 49, at Congleton 26, Macclesfield 26, Dr. Bellyse, of Oakfield, says "that the soil is partly sand, but generally clay." Dr. Bellyse thinks that the general impregnation of the soil with salt (chloride of sodium) accounts in some measure for the comparative immunity from phthisis. At Macclesfield and Congleton the lower orders are engaged in silk mills, and at Macclesfield there are many flour mills. At Nantwich, the lower orders are chiefly employed in shoemaking, and the wages are tolerably good."

At Stonehouse, Devonshire, the great mortality (27) is, to some extent, accounted for by the presence of the Naval Hospital, and by the number of invalided seamen. Mr. Perry, of Stonehouse, informs Dr. Dale, from whom I obtained this information, "that

Mr. Rodd, the Superintendent Registrar, believes that two-thirds of the cases are imported from abroad." Dr. Dale says that the drainage is carried into "large chasms of the rocks," a method, I should think, not likely to add to the salubrity of the town.

Dr. Buchanan, of Haltwhistle, Northumberland, where the death-rate from phthisis (35) is the lowest in that county, believes that many deaths are uncertified, and are improperly registered under the head of consumption. The district is thinly populated, and a large portion of it is totally undrained. The people are generally a hardy, vigorous race, employed chiefly in sheep farming; there are some woollen manufactories and coal mines in the district, with lead mines on the border, but the mines here are very free from phthisis." As the table shows, at Glendale, in the same county, the mortality from this disease is only 61, and at Belford, 64.

Dr. Nicholls, of Devizes, believes "that a moist atmosphere is conducive to the production of phthisis, and that a dry atmosphere checks a tendency to that disease. Salisbury was extremely damp; since its better drainage phthisis has decreased in a marvellous manner. Alderbury district (34) is damp and badly drained. Devizes (34) and its neighbourhood badly drained, upper green sand. Tisbury (53) well drained on Portland oolite, full of fissures. Malmsbury (52) on a slope, well drained. Westbury (48), upper green sand on a slope. The places most exempt from phthisis are much drier."

At Redruth, Cornwall, including Camborne and Gwennap, the mortality from phthisis is as low as 27, the population 55,401; the number of acres to each person ·73, and the general mortality for twenty years 21 per 1000. Mr. H. Harris, surgeon, of Redruth, has furnished me with much valuable information, which I regret that I am obliged greatly to condense. "It rains here sometimes for six or eight weeks, and, on the other hand, I have known it for as long a time dry and dusty, but it may be called a moist, damp, wet country. The miners have generally a poor diet, chiefly salt pilchards and bread, and a few potatoes for some of their meals. The wages of some are too small to allow of any other animal food than fish. Then wet and cold underground work is baneful; and the climbing perpendicular ladders 250 fathoms cause, sometimes, great palpitation of the heart and congestion of the lungs, sometimes followed by spitting of blood. Early marriages also produce an injurious effect."

WALES.

As before observed, pulmonary consumption is fearfully prevalent in this part of the United Kingdom. I shall, therefore, devote as much space as I am able, to the correspondents who have kindly furnished me with the subjoined information.

Dr. D. W. Williams, of Menai Bridge, Bangor, who has resided there for twenty years, and for eight years had the Bangor and Beaumaris Union, believes "that the deaths are so imperfectly registered that little dependence can be placed on the statistics, and that until medical registrars are appointed no satisfactory results can be arrived at, many deaths being attributed to phthisis that do not appertain to that disease. Anglesea (27), from its considerable elevation above the level of the sea, being nearly all table-land, with a clear bracing atmosphere, light soil and gravelly or rocky bottom, cannot be the hot-bed of phthisis that the Registrar General's Returns would prove it to be." Dr. Williams admits that some parts are badly drained, and that marriages of consanguinity have an injurious influence in the production of phthisis.

Dr. Williams, Penygroes, Carnarvon, (26) says, "We have here, Landurvy, about 3000 men employed in the slate quarries; they live hard, tea, coffee, and bread and butter, varied occasionally with bacon and potatoes, forming their chief subsistence; early work, early marriages, close and ill-ventilated apartments, long walks to their labour, and other causes, all tend to explain the great mortality from this disease. Bangor (29) and Conway (34) I believe are similarly situated, having some thousands of men employed in the quarries. Pwllheli (41) is more of an agricultural district. I believe that the deaths are pretty fairly registered, and that the disease is genuine tubercle."

Dr. Griffith, of Portmadoc, Carnarvon, gives nearly a similar account to the above. In the mining districts, he says, "they get but little milk, and live chiefly on bread and butter and tea, and very little meat. Beer is taken irregularly, and often in excess. In the agricultural districts (Pullheli) they get milk and meat, the latter chiefly salt. The difference in the mode of living accounts for the rate of mortality more than the occupation I believe, as women and children fall victims to the disease as often as the men. It is genuine tubercle, and, singular enough, in the Festinroy slate

district, which I often visit, pneumonia attacks the *apex* as often as the *base* of the lung, and many so affected become phthisical."

Through the kindness of Dr. Sheen, of Cardiff, Dr. Evans, of Tynant, has supplied me with the following—"He does not believe that the climate of Wales conduces to the production of phthisis in any marked degree, but that the undrained soils in many parts, and the deep valleys of Wales may have contributed, as in other localities, to increase the mortality from this cause. Phthisis during the last fifteen years has diminished at Cardiff, in consequence of the prevention of overcrowding, and better attention to hygienic measures. Bad nursing, scarcity of milk, bad construction of houses, working under ground in hot works at an early age, intermarriage, early marriages, drinking, smoking, and indulgence of the passions among the young, have all tended to promote the increase of pulmonary consumption, as they have served to swell the number of deaths from zymotic diseases." Dr. Evans's testimony is especially valuable, as he lives in the heart of the mining district.

Dr. W. W. Morgan, of Newport (37), Monmouthshire, believes "that many cases of chronic bronchitis, and even of acute bronchitis, are carelessly put down for phthisis, and also some cachectic forms of disease. Merthyr, where the mortality from phthisis is as low as 24, is the highest district, and until of late without sewerage or water, except from wells and the river, into which the surface drainage of a wide district flows. Dr. Morgan describes the peculiarities of Abergaveny 30, Newport 37, Pontypool 39, Chepstow 43; and describes Monmouth, where the mortality is only 49, as having a great advantage over the other towns, there being no large manufactories to contaminate the air; the population is less dense, there is less dissipation, and the houses generally are not so close and ill-ventilated." I scarcely need say that the above towns are in Monmouthshire, a county often united with Wales, and it is remarkable that, although the deaths from phthisis are less than in any of the Welsh counties, the *general* mortality is greater than in Wales. Thus, in Monmouthshire, the number of acres to each person is 2·31; in South Wales, 4·31; in North Wales, 4·81; the general mortality in Monmouthshire, 23 per 1000; in South Wales, 21, in North Wales, 20 per 1000. The large amount of rain-fall in Wales is likewise an important fact in relation to this inquiry.

Want of space prevents me, on the present occasion, from

entering into other important matters connected with phthisis, such as climate, geological formation, fungoid origin of the disease, mode of prevention, treatmeat, etc.

NOTE.—Some arithmetical errors occurred in my last paper, in consequence of the hasty reading of the proof. Read as follows:—St. Olave, 27 *for* 11; St. George's East, 27 *for* 28; Lewisham, 54 *for* 34; St. Pancras, 34 *for* 36; Stepney, 38 *for* 37; Kensington, 35 *for* 36. Manchester, 26 *for* 27. Isle of Wight, 38 *for* 37. Gravesend, 37 *for* 38. Glendale, 61 *for* 60. Tenbury 63. Pershore, 54. Knighton, 59 *for* 61.

The estimates in the present essay have been carefully made by myself.

ON THE MOVEMENTS OF THE IRIS.

By C. Eckhard, M.D., Prof. of Physiology in the University of Giessen.

Within the last ten years the movements of the iris have been studied with special care, many points have been settled, whereas others still wait for their final decision. The different papers bearing on this subject being rather scattered, as is commonly the case in our branch of literature, I thought it might be of some use, to give a condensed and critical account of them. I am the more inclined to do so, as lately I have had a chance of occupying myself with this subject by my own experiments. I omit, of course, the treatises of remoter days; those who like to follow the whole history of this theme may take in hand the papers composed by Professor Weber* and Professor Budge.† Stepping directly in *medias res*, I shall treat separately the following points on which there is some dispute.

1.—*Structure of the optic commissure.* According to the reports of anatomists, there is no doubt that in certain animals, especially in fishes, both of the optic tracts undergo a thorough decussation within the optic commissure. As to the higher vertebrate animals some doubts may be entertained in this respect; at least, so long as the method employed is merely an anatomical one. In man anatomical researches, as it is sufficiently known, have led to the opinion that the optic tracts undergo only a partial decussation. It is true that Dr. Biesiadocki‡ has tried to show that the true structure of the optic commissure is the same in man as in all the vertebrate animals, viz., complete decussation; but I do not think that the mere microscopic examination will ever form a sufficiently stringent demonstration of Biesiadocki's doctrine. Many anatomists, whose opinion I have asked, proposed to leave it to every one's own taste, how far his conviction in this respect might go.

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* Programmato collecta. Fasc. iii. p. 1.

† Uber die Bewegungen der Iris, 1855.

‡ Moleschott's Untersuchungen, Bd. viii. p. 156.

I myself have often examined the commissure, and confess that there have been left many doubts in my mind, for I could not trace the course of numerous fibres sufficiently clearly.

At all events we ought to look out for physiological facts; for these may, perhaps, settle the question in dispute. As to man, there are as yet three classes of facts only which have been considered. First, the changes taking place in the course of the optic nerve and optic tract after extirpation of the eyeball or in blindness of one side; second, the phenomena of hemiopia; and, thirdly, the movements of the iris, when one of the retinæ is irritated by light or in any other way. The facts of the first series are of no service, they cannot be compared with each other, for they are not alike. In one case there is reported a partial atrophy of the optic tract of the same side, in another of the opposite side, in still another of both sides, and again in another only of the optic nerve between the eyeball and the commissure, but not beyond the latter. It must also be admitted that these reports may not be in other respects of the same value, for they belong to different periods of our science, and consequently they may not have been treated with the same due care. In our own days, where we are more accustomed to and disciplined for careful examinations, we should not let pass any chance we might get of continuing these investigations. The cases of hemiopia, as far as I myself am acquainted with them, afford no better clue. The phenomena during life are sufficiently described, but I know of no case in which the post-mortem examination showed an exclusive degeneration, etc., of the optic tract or optic nerve. I know very well that in some books one meets assertions of this kind, but no references whatever are given. Dr. Rüdinger for instance, in his book on the cerebral nerves, says, page 8:—"Diese anatomischen Thatsachen, (viz., demidecussation in the commissure,) wurden mehrfach bestätigt. Eine Geschwulst, welche z. B. auf den rechten tractus opticus einen Druck ausübt, erzeugt functionelle Störungen der rechten Retinahälften auf *beiden* Augen;" but he does not refer to any authority. As to the contraction of the pupil after irritation, we know that in man the pupils of both sides contract when only one retina is excited. The phenomenon is the more precise the less the pupils are contracted previously. In order to get the contraction independently of accommodation we must fix both eyes on the same object, and during this time throw light from the side into one eye. It is clear in itself that this experiment does not distinctly show that

only a partial decussation takes place in the optic commissure, for it may be explained quite as well by presuming, that each optic tract is connected within the cerebrum with the common motor of both eyes. We ought to have such an experiment as would represent an irritation of the optic tract. There are pathological cases of this kind reported, but most unfortunately they do not afford strong evidence for either of the two contending views. Let us take, for instance, a rather good case I met with the other day in the "Archives of Medicine," by Professor Beale.* There Dr. Bateman records a case of cholesteatoma, in which the tumour was found lying against the under surface of the right lobe of the cerebellum, and the anterior extremity of which pressed upon the posterior and inferior surface of the right thalamus. During life, amongst several other symptoms, double amaurosis was observed; but it was not complete, as light and darkness could just be distinguished. Both pupils acted sluggishly; there was no apparent disease of the eye itself. No doubt, one is inclined to refer the double amaurosis to a partial decussation of the optic fibres within the commissure; Dr. Bateman himself reasoned in this way. But by the fact that the tumour pressed against the optic thalamus itself and not exclusively on the tract, it is presumable that the pressure on the thalamus of the one side was propagated to that of the other side; or, we may also imagine, that within the thalamus of each side central parts of vision for both sides are located. The consequence is that we got no clear case. I have examined a great many cases of a similar nature, but when criticising them, I met at all times obstacles of different kinds, which never would allow me to come to a definite conclusion. From the preceding observations it appears that till now neither anatomy, nor physiology, nor pathology, can clearly demonstrate the true nature of the intimate structures of the optic commissure, although partial decussation is the most plausible presumption. In animals the physiological method may be employed on a larger scale. Some months ago I had an opportunity of forming an opinion from my own experiments. A pupil of mine worked in my laboratory, and I recommended him to experiment upon the dependency of the movements of the iris on the corpora quadrigemina.† At the com-

* Vol. iv. p. 316.

† Knoll: Beiträge zur Physiologie der Vierhügel. V. iv. of my contributions to anatomy and physiology.

mencement of his investigations, in which I took an active part, we were obliged to observe in what manner the pupils of both eyes act when only one optic nerve is irritated. We used rabbits, and laid bare the optic nerve on one side within the skull; for the method employed I refer to Dr. Knoll's paper. We learned that when the optic nerve of one side is irritated by the electric current, contraction of the iris of the same side only takes place, the pupil of the other side remaining unaltered. In these experiments we observed further that light thrown into one eye produced contraction of the pupil of the same side only, provided, of course, that the illumination of the other eye did not change during the experiment. To demonstrate these facts we used either albinos, the pupils of which act rather precisely when the eye is alternately shaded from or exposed to daylight; or, when we were constrained to use rabbits of some colour, the pupils of which act very sluggishly, we employed artificial light, as, for instance, Dubosq's lamp. It is true these facts do not prove that there is complete decussation within the commissure, but they show that the optic nerve of one side is in no correlation whatever with the motor of the iris of the other side, but the test of decussation can now be easily applied. When the optic tract is cut anywhere on its course between the cerebrum and the commissure, and the cerebral stump is afterwards irritated, contraction of the iris on the opposite side only is observed. Now this observation decides the question, and proves evidently that in rabbits, and most probably in many other quadrupeds, a complete decussation takes place within the commissure.

But in spite of this apparently determinate result, I hesitate still to acknowledge these facts as conclusive. The reason why I am so sceptical lies in my not knowing if those fibres which acted in the above-mentioned experiments as inciting ones for the reflex movements of the iris are the same as those which convey the sensation of sight. Supposing that there exist two kinds of fibres in the optic nerve and tract, the one serving for inciting reflex movements of the iris, the other for producing sight; the experiments related would then show that only the first kind of fibres undergo no decussation; as to the genuine optic fibres nothing is proved by them, and I may still presume that the last mentioned group of fibres partially decussate. There we meet a lacuna; for on this head I know of no experiments cautiously executed on animals, and I myself have till now not found any leisure to devote myself

to such researches. As to man, there are some pathological facts which one might recall, but they give no evidence whatever. The kind reader guesses already to which ones I am alluding; of course, I am thinking of those cases in which sight was lost in consequence of cerebral apoplexy and the movements of the iris still continued to respond to light. Every one is acquainted with the cases reported by Professor Graefe,* and to many practitioners similar ones may have occurred. I myself have seen such a case. One eye was totally blind, the pupils of both sides acted well, the ophthalmoscope showed no symptom in the eye itself; all the muscles of the eyeball acted precisely; but there was some cerebral alteration declaring itself by partial deafness, loss of sensitiveness of the arm, etc. When I examined the play of the irides by throwing different quantities of light into the affected eye, I took special care not to allow the eyeball to move, in order not to be deceived by any movement of the iris which might take place simultaneously with the action of the other muscular fibres of the common motor of the eye. When I illuminated the blind eye the pupils of both eyes contracted themselves, as if there was no disturbance at all. I need hardly say that all these cases do not admit of any conclusion as to the true nature of the fibres still in action. Although it would seem as if the genuine optic fibres were different from those engaged in the reflex movements, we ought not to rely upon such a presumption, for we may quite as well assume, that both processes may be served by the same nerves, with the sole exception that these are connected with different parts of the cerebrum.

2.—*Cerebral centre engaged in the reflex movements of the iris.* After the researches made by Professor Budge and others, there can be no doubt that the centre of the reflecting movements of the iris lies in the corpora quadrigemina, but there exists neither perfect accordance as to the facts, nor is the same idea entertained how we ought to explain the different states of the pupils after the corpora quadrigemina have been destroyed. As to the facts, some of them could not be confirmed by the experiments lately made in my laboratory. The following statement I consider to be correct:—

a. The posterior corpora bigemina and the thalami optici have, as is previously shewn by the experiments of others, nothing to do

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* Dessen Archiv. vol. 1., p. 266.

with the movements of the iris. One can take them away, and yet the iris plays unaltered in accordance with the degree of its illumination.

b. The anterior corpora bigemina can be destroyed for *the most part,* and the iris keeps still its movements. It is only a very small part of about one millemetre in breadth at the antero-internal limit, and of course the optic tract itself, that must be left untouched. Professor Budge has arrived at nearly the same result; he differs slightly in so far as he gives the essential part a greater extent than Dr. Knoll.

c. If one of the two parts just mentioned is cut across, the pupil of the other side becomes dilated and paralysed, and it is impossible to produce reflex contraction of the iris by any means whatever.

d. If the antero-interior part of the anterior bigemina is irritated by the electric current, contraction of both pupils results. As it is almost impossible to limit the current to one side, we must at present abstain from a more profound discussion of this point; and I mention this observation at all only that I may have an opportunity to rectify the current statements on the consequence of an electric irritation of the anterior bigemina. Commonly, it is said, that this procedure engenders *contraction* of the pupils, but we can by no means say so, for the electrical excitation of by far the larger part of the anterior bigemina will not result in any contraction of the pupils; quite the reverse, dilatation follows. Dr. Knoll and I were rather astonished to meet with such a result; for since the reports of Flourens and Longet, one was so much accustomed to their doctrine as not even, by any possibility, to think of the opposite. As often as we have repeated the experiment with that care, neither to touch the spot mentioned above, nor to use currents of such an intensity as to produce general convulsions, we got the same result. Then the question arose, could the sympathetic fibres be traced up to the corpora quadrigemina. We thought this likely to be so, as Dr. Salkowsky* has already met with them in the medulla oblongata. Now we repeated the experiment, after having previously cut the great sympathetic in the neck, and true, no dilatation could be seen. From this experiment we must conclude that those fibres of the great sympathetic nerve which dilate the pupil, ascend within the cerebrum as high up as the anterior bigemina. Whether

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* Henle's Zeitschrift, Bd. 29. Heft. 2.

there be any decussation of the sympathetic fibres, as one might conclude from the observation, that the irritation on one side produces dilatation of both pupils, I will neither assert nor contradict, for I know too well how fallacious the conclusion would be, solely derived from the effects of electrical irritation. It may, perhaps, be said, But why not try some other irritation of no such deceptive character, and solve the question in this way? I thought so too, but using a strong solution of chloride of sodium I observed no effect whatever. I shall touch upon this point hereafter.

The authors who have occupied themselves with experiments on the centre of the reflex movements of the iris, differ also respecting the idea they entertain as to the manner in which dilatation and paralysis of the pupils are produced, when the anterior bigemina become destroyed. Some of them refer these phenomena to the destruction of the reflecting centre itself, others* regard them as mere consequences of the lesion of the optic tract, pretending that as at present the anatomical position and physiological nature of the said centre are not sufficiently known, and as it is impossible to spare the optic tract when operating on the anterior bigemina, the nerve penetrating into the antero-internal part, there is no sufficient reason not to take first into consideration the tract itself. I do not believe that this difference of meanings matters very much, for as long as we have got no clear conception of all the properties of the reflecting centre, and know not how it is connected with its nerves, we are unable to infer from certain phenomena in what way they in reality were produced. Dilatation and paralysis of the pupil may perhaps be produced by cutting the optic tract, as well as by destroying that spot where it is connected in some, but a still unknown way with the common motor of the eye. In consequence thereof, in a given case of dilatation and paralysis of the pupils, we can say nothing respecting the manner in which it was produced. It is only as long as we can trace undisputedly the optic tract, that we are entitled to refer the said phenomena to this part when cut. But as the optic tract, where it merges into the antero-internal part of the anterior bigemina, can be no longer followed sufficiently clearly, the effects of a destruction of this part will remain obscure. There is no sufficient reason to prefer either of the two

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* For instance, Pietro Renzi:—"Saggio di fisiologia sperimentale sui centri nervosi della vita psichica nelle quatro classi degli animali vertebrali." Annali Universali di Medicina, volume 190. 1864.

explanations offered; and in the present state of our knowledge there is no weighty difference between them.

3.—*The physiological properties of the nerves and muscles of the iris.* The general view under which these constituents of the iris were hitherto considered, consisted in viewing the third pair as the simple motor nerve of the sphincter pupillæ, and the grand sympathetic as the like motor nerve of the dilatator iridis, each of them provided with certain physiological properties serving as grounds to explain the facts we are at present acquainted with. But, in consequence of modern observations, it has been proposed, to leave that old sanctioned opinion, and to proclaim a more fashionable one. Before citing and criticising these novelties, I will try to gather all the facts relating to the properties of the parts concerned in the movements of the iris. In doing so I shall not neglect to add my own experiments, and to show how far the facts in reality go. It seems to me that some of them have been illused, having received an interpretation which they will not bear when closely examined.

a. The section of the sympathetic in the neck does not essentially alter the movements of the iris in its dependency upon light and shade. As far as I know, Dr. Grünhagen* was the first to lay some stress on this point. He said that it makes no difference whether one takes away a small piece of the nerve or the upper ganglion of it, the movements of the iris remaining on the whole the same. I myself have repeated these experiments, and planned some other ones, the bearing of which lies in the same direction. My report is this. After having cut the sympathetic in the neck, the pupil does *not contract*, of course only so as long as the degree of illumination of the retina remains the same. All assertions of an opposite character I do not consider to be true. This observation shows that the nervous centre, where the sympathetic originates, does not maintain in this nerve an irritation of any palpable degree. There is another experiment, the execution of which I recommended to Dr. Knoll, which tends to prove the same inactivity in relation to the origin of the common motor of the eye. It consists in cutting first the optic nerve, after which operation one gets dilatation, and then the oculomotorius. But now no further dilatation is seen, although the pupil is still capable of it, for when the sympathetic is irritated dilatation promptly occurs. Both experiments are in

* Virchow's Archiv. Bd. xxx. p. 481.

accordance with the modern conceptions respecting the tone of the cerebro-spinal system. When the sympathetic is cut, and the retina is darkened and illuminated alternately, the pupil still acts, so that one might get the impression that no injury had been done to any part in connection with the eye. And again, when one dilates the pupil to gradually increasing extents, using for this purpose corresponding intensities of the induction current, and then as before darkens and illuminates alternately the retina, at each gradation of enlargement it will be seen again, how the iris moves corresponding to the degree of its illumination. Of course, there is a limit, for if the pupil has got its greatest dilatation by using currents of high intensities, the diameter can augment no more, when the retina is still further shaded. I am not quite certain if the limit extends exactly to this degree; I incline more to the idea that it lies near that point, if by electrical irritation of the sympathetic the pupil has got a diameter nearly the same as it gets when, in the absence of any irritation, the retina is shaded. These observations show that there is a dilatation of the pupil corresponding in its different degrees to those of the shadings of the retina, and which are not produced by the aid of the grand sympathetic.

b. The second series of facts I will group around the long known atropina experiment. Most of them originated in the attempt to explain which parts might be affected by this alkaloid. The first endeavour of this kind was made by Dr. de Ruiter* under the auspices of Professor Donders. As in this paper it is impossible to cite every experiment communicated by any physiologist, I confine myself always to those which possess such a predominant character, that they were the decisive reason why the author was led to the idea he promulgated. As to those experiments which are of a mere additional character, I omit them, trusting that those who intend to promote our doctrine will consult the originals. Now, to return to the paper of Dr. de Ruiter; the author is of opinion that atropina paralyses the sphincter pupillæ, and irritates the dilatator, or diminishes the forces of the oculomotorius while it excites those of the grand sympathetic. To this supposition Dr. de Ruiter was principally led by the observation that atropina still produces

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* De Ruiter: De Actione Atropæ Belladonnæ in Iridem, 1853. Also Donders: Onderzoekingen gedaan in het Physiologisch Laboratorium de Utrechtsche Hogeschool, 1853-54, p. 83-122.

dilatation of the pupil, even if the cerebrum is destroyed, and the heart cut out. In 1856 Dr. Bell* expressed the opinion that atropina might perhaps dilate the pupil by irritating solely the grand sympathetic. To test this hypothesis Professors Sharpey and Harley† suspended the upper end of the cut sympathetic in a strong solution of atropina, but got no dilatation. I do not think that this experiment does conclusively destroy the theory of Dr. Bell, for physiologists know that nerve trunks may comport themselves differently to their finer ramifications within the tissues. Neither can I agree with the mode by which Dr. Grünhagen‡ tries to oppose the view that atropina excites the fibres of the sympathetic. It is true his argument rests upon a fact, the correctness of which I can corroborate, that after the pupil has been dilated by atropina, we get a still greater dilatation, when we irritate the grand sympathetic at the neck; but, it is obvious, that no fact restrains us from the presumption that the alkaloid might affect the filaments of the sympathetic which ramify in the iris to a certain degree, and that such an irritation is no reason why a stronger excitation, acting upon the trunk of the sympathetic, should not produce dilatation to a still higher degree. I do not care if any one finds my reasoning too sceptical and hair splitting; if our doctrine is to be truly advanced, we must not rely upon probabilities. But supposing that the dilatation by atropina be not produced by irritation of the sympathetic, the question arises what parts does the alkaloid affect. Drs. Bernstein§ and Dogiel have given the answer; by paralysing the fine ends of the oculomotorius within the iris. They deduced this theory partly from Dr. Grünhagen's experiments, but mainly from the observation, that after having administered atropina, and having got the usual dilatation, any irritation of the oculomotorius within the skull no longer produces contraction of the pupil, while at the same time the sphincter pupillæ will contract itself when the electric current is applied directly to the sphincter iridis. The first part of their experiment shows the inactivity of the oculomotorius, the second that the sphincter iridis still keeps its vital properties. Yet

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* Report of Cases at the Edinburgh Eye Infirmary, by R. Hamilton and B. Bell, Edinburgh Medical Journal, July, 1856.

† Edinburgh Medical Journal, November, 1856.

‡ Hermann's Centralblatt, No. 37, 1863.

§ Hermann's Centralblatt, No. 29, § 453, 1866; and Henle and Pfeiffer's Zeitschrift, Dritte Reihe, Bd. xxix. p. 33.

I do not believe that these observations affirm undoubtedly the theory of Drs. Bernstein and Dogiel. I admit that as they have found irritable the oculomotorius on the sound side, *i.e.* where they had not applied atropina upon the conjunctiva, the conclusion may be to a certain degree justified, that the oculomotorius of the other side was paralysed in its endings; but I cannot consent to the inference if, from the contraction of the sphincter by direct irritation of its tissue one takes the liberty to conclude that the alkaloid had in no way paralysed the sphincter, for the latter may have lost only a portion of its contractility, and so still be able to contract when a strong stimulus is applied to it. So it appears to me that although I myself acknowledge the facts related by Sharpey and Harley, Grünhagen, Bernstein and Dogiel, there is not one observation which can stand as an unattackable witness for or against one of the contending views respecting the part affected by atropina. But there is still one point to touch upon. Since it has been made known that in the iris, and in the parts adjacent to it, ganglionic cells* exist, we are in a still greater uncertainty about the parts on which the atropina acts. This is the reason why I conclude this part, by alleging that although it is highly probable that atropina attacks the ends of the oculomotorius, sufficient scientific proofs of this theory are still wanting. I have often reflected on this matter, and tried to invent an *experimentum crucis*. To what extent I have succeeded I leave the severe critic to decide. Suppose we cut the sympathetic in order to avoid any dilatation of the pupil through the channel of this nerve. Then we cut the oculomotorius, or what, as Dr. Knoll has shown, is of the same service, the optic nerve. After having measured the diameter of the pupil, we apply atropina, and measure again a certain time after its application. If we find the pupil larger at the second measuring, we must conclude that atropina effects something more than simple paralysis of the oculomotorius. I have executed many experiments of this kind, but the result was not always the same. On white rabbits I found, in most cases, a small augmentation of the diameter after the use of atropina; in those of other colours no striking result was obtained. If I compare these facts with

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* The reader will kindly excuse the following references: H. Müller, Würzburger Verhandlungen x. p. 137; Schweigger, Graefe's Archiv. Bd. v. und vi.; Arnold, Virchow's Archiv. xxvi. p. 345; Krause, Anatomische Untersuchungen, Hannover, 1861.

De Ruiter's statement, having seen the effects of atropina on eyes of animals of which the heart was taken out and the cerebrum destroyed, I incline to believe that our famous alkaloid produces a state in the tissues of the eyeball, which is not exactly identical with paralysis of the oculomotorius. Still the experiments ought to be repeated. Meanwhile we make no essential mistake if we say, that atropina acts preferably on that mechanism, by the aid of which the oculomotorius contracts the pupil.

c. The last series of experiments comprise those made in using the curara and calabar bean. As to the effects the curara produces on the movements of the iris, those physiologists who have worked on this subject are somewhat in accord. Dr. Gianuzzi* observed: *a.* that in dogs poisoned by curara, to such a degree that induction currents applied to motor nerves no longer produced contraction in the limbs, and kept alive by artificial respiration, irritation of the grand sympathetic still produced dilatation; *β.* that the pupils of such animals still contract when irritated by light; *γ.* that atropina applied to the conjunctiva of such an animal still dilates the pupil; *δ.* that such a pupil contracts when the sympathetic nerve is cut. Dr. Keuchel's† experiments gave the same results. When he galvanised the sympathetic he got dilatation, and then the common motor of the eye being irritated he saw the pupil contract while the muscles of the eyeball remained in their relaxed state. I myself am fully convinced of the correctness of these statements; it is only the last one of Dr. Gianuzzi that I still entertain some doubts about. As to calabar bean, its myotic effect, discovered by Professor Fraser in 1862, is sufficiently admitted; but respecting the idea that is entertained of the parts it attacks, the opinions widely differ. Although I almost believe that, at present, no experiment has been planned to settle the question, I will discuss the matter, that perhaps we may get the conviction of the inefficiency of the promulgated theories, and be induced to look for such facts as may bear criticism. One of these theories consists in supposing that the sympathetic is paralysed.‡ It is based on two facts: first, irritation of the sympathetic produces either only a very small dilata-

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* Medicinisches Centralblatt, 1864, p. 321. See also Rogow in Henle's Zeitschrift, Dritte Reihe, Bd. xxix. 1, p. 9.

† Das Atropin und die Hemmungsnerven, Dorpat, 1868.

‡ Bernstein and Dogiel, Centralblatt für die Med. Wissenschaften, 1866, p. 453; partly also Hirschmann and Rosenthal, Reichart and Du Bois-Raymond's Archiv., 1863, p. 309.

tion or none at all; second, after having previously paralysed the sphincter iridis by atropina, one gets by the application of the calabar bean, a medium width of the pupil. I myself cannot adopt this opinion. First I again call to mind the ill-use one makes of that imagined paralysis when the sympathetic has been cut. I persist in saying, that as the sympathetic, when cut in the neck, produces no contraction, although it contains a very large amount of dilating fibres, I cannot conceive why it should be otherwise when paralysed at any other point of its course, even if there it contains some more dilating fibres. We cannot make any assumption where the simplest fact is against us.

Moreover the difficulty of producing dilatation of pupils contracted by calabar, by applying the current to the sympathetic, can easily be explained; for it is obvious that in this case the dilating force of the irritated sympathetic meets with a considerable resistance exerted by the contracted sphincter, as I will show afterwards. If it be said that the irritated sympathetic produces no dilatation at all of pupils contracted by calabar, I contradict the statement. The true fact is, that we must take refuge in stronger stimulants. I know an experiment, in every respect similar to the one described above. Lay bare the sympathetic at the neck, open the skull, and contract the pupil by irritating the trifacial as much as possible. Now try to enlarge this pupil by irritating the sympathetic, and it will be found that, in surmounting the resistance of the contracted pupil, stronger currents than before have to be employed. If you use currents of which the intensity is greater than necessary to dilate the pupil *ad maximum* in the sound eye, of course you can dilate the contracted pupil without becoming aware that any resistance exists. This is the reason why some physiologists say that pupils contracted by calabar can be quite as much dilated. As respects the second reason, the statement of the fact ought to be framed in this way: If atropinised pupils are treated with calabar the effects vary according to the doses used and the time each is allowed a chance to act. Then, we meet, undoubtedly, with cases in which calabar produces no contraction at all within the time that it takes to affect the sound eye. To get such a result the paralysis of the oculomotorius by atropina must be very complete and of some standing.* In other cases one observes a certain

* See also Rogow, l.c. p. 11.

contraction, the degree of which depends upon the circumstances already mentioned; but in most cases of pretty-completely atropinised eyes the contraction is insignificant.* Of course if calabar acts profusely, and for a long time, on mydriatic pupils you may get even full contraction. In consequence of these reflections I do not see any reason to adopt the hypothesis that calabar contracts the pupil by paralysing the sympathetic.

The other hypothesis considers the contraction of the pupil by calabar as a state of irritation of the sphincter. In general, I most energetically side with those who have made this theory their own, excepting a slight deviation from the idea they have formed as to the manner the contraction might be produced. Dr. Robertson† believes that calabar contracts the sphincter by irritating the ciliary nerves. Not to say anything about the not well-selected expression of ciliary nerves, for their composition has not been made out, inasmuch as we do not know how far the trifacial and the oculomotor participate in it, I am of opinion that the manner in which Dr. Robertson tried to found his theory was not convincing. In this respect I agree with what Mr. Nunneley has advanced.‡ Dr. Harley, Professor Gräfe,§ Dr. Grünhagen, ‖ and Dr. Rogow¶ agree with Dr. Robertson, so far as they reduce the effect of calabar to some irritation of the sphincter. At present our position is this. First, we know of no contraction of the pupil after paralysis of the sympathetic, and especially not of such a high degree of contraction, as is observed after the application of calabar. Second, the sympathetic is not paralysed by calabar. Thirdly, there is no other way of accounting for contraction than the presumption of some irritation of the sphincter. Now arises the question, in what manner this contraction can be produced. There is the sphincter itself, the common motor of the eye and the ganglionic centre it is connected with, and finally the trifacial, we may believe implicated in this process. As undoubtedly the effect of calabar is local, we must abandon the idea of distinguishing accurately between

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* Graefe: Archiv für Ophthalmologie. 1863.

† "The Calabar as a New Ophthalmic Agent."

‡ Lancet, 1863, ii., p. 589.

§ Archiv. für Ophthalmologie, 1863.

‖ Virchow's Archiv. Bd. xxx, p. 481, and Berliner Klinische Wochenschrift, 1865, pp. 242 and 252.

¶ L.c.

any irritation of the nerve-ends and the muscular fibres of the sphincter. Consequently, it will be rather difficult to make out, if the common motor of the eye, or the trifacial is affected by the poison. Still there are reasons to entertain the belief that the system of the oculomotorius is the one attacked. Atropina paralyses the oculomotorius and leaves the trifacial uninjured; for irritation of the trifacial after dilatation by atropina produces energetic contraction. Now if calabar would attack the trifacial there ought to be the same effect in both of two pupils, of which one is paralysed by cutting the optic nerve, the other by the application of atropina. But this is by no means the case; for whilst in the same time by the use of the same quantity of calabar the atropinised pupil contracts very little or not at all, the other shows contraction to a considerable degree. Again, if the trifacial is the point of application I could not understand the fact that calabar renders the eye myopic, for, as far as our present knowledge reaches, the adaptation of the eye depends upon the oculomotorius. I cannot help observing that I make no use of the myopic effect of calabar to derive from it the manner in which calabar acts, as Dr. Robertson did, but that I only allude to it as a fact in accordance with the view proposed. In writing this another experiment presents itself to my mind. Perhaps the trifacial nerve loses its influence on the pupil in animals poisoned with curara. If this be be the case, and the promulgated theory true, calabar will still contract the pupil in animals poisoned with curara and kept under artificial respiration. I recommend the execution of this experiment, and the more so as there is another one like it, which, if it can be trusted, would be in opposition to the theory. Dr. Rogow* dilated pupils by atropina, then he poisoned the animals with curara, and finally injected subcutaneously calabar. He observed no effect. As curara does not paralyse the oculomotorius I would have expected contraction of the pupils. The experiment ought to be repeated, and calabar locally applied.

After having collected the most important experiments lately made to elucidate the physiological properties of the parts concerned in the movements of the iris, I will now give an account of the different theories planned to explain the actions of the sound pupil. Amongst these I cite:—

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* L.c. p. 14.

a. Dr. Grünhagen's Theory. The author * imagines that there is no dilatator iridis, and that, consequently, the dilatation must be explained in another way than it hitherto has been. But it seems as if this novelty does not rest on firm grounds. Some anatomists have attacked its author, and I believe that at present Dr. Grünhagen's theory is almost abandoned. Henle,† Markel,‡ and Hüttenbrenner,§ saved our muscle, although they showed that its position and the arrangement of its fibres do not exactly correspond with the description formerly given. The muscle consists, as Henle first pointed out, and his successors have corroborated, of a continuous layer, very near the posterior surface, where it is only covered by cells. Dr. Grünhagen, in his attempt to explain why irritation of the sympathetic nerve produces dilatation, in his first paper said that this phenomenon was produced by an alteration of the intraocular pressure, afterwards‖ he abandoned this hypothesis, and regarded the dilatation as the consequence of a contraction of the blood vessels within the iris. I do not find this explanation sufficiently lucid, and leave it to the reader how far he inclines to be contented with it. Some points of minor interest in Dr. Grünhagen's theory I will here omit. I shall touch upon them hereafter.

b. V. Bezold's Theory. V. Bezold inclines to believe¶ that the common motor of the eye and the sympathetic are not connected in the same way with the respective muscles, the first most probably being connected with the sphincter pupillæ by some intermediate ganglionic link. To this intercalated part he gives no proper name. I mention this fact because Dr. Keuchel,** in his opposition to V. Bezold's theory, writes as if V. Bezold had expressly meant

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* Medicinisches Centralblatt, 1863, Nr. 37, and Virchow's Archiv. Bd. xxx. p. 481.

† Dessen Jahresbericht, 1864, s. 129.

‡ Henle und Pfeuffer's Zeitschrift Bd. xxxiv. p. 83.

§ Sitzurgsberichte der Wiener Academie Math-Nat. Classe, Bd. lvii. p. 515.

‖ Virchow's Archiv. Bd. xxx.

¶ Promulgated in Sitzurgsberichte der Physikalisch-medicinischen-Gesellschaft zu Würzburg, 12 Febr. and 16 May, 1866; Fridericus Blocbaum de vi Physiologica atropini sulfurici Diss-inaug-Gryph.; and Natersuchungen aus dem physiologischen Laboratorium in Würzburg; v. Bezold, 1 Heft. 1867, p. 67.

** L.c. p. 77.

the ophthalmic ganglion. Regarding the sympathetic he presumes that it is not connected with any ganglion, but goes directly to the fibres of the dilatator. By the aid of this supposition, and supported by experiments he made in applying atropina to other organs, especially the heart and the intestines, he argues in the following manner. Atropina paralyses first the ganglia, with which the oculomotorius is connected; in larger doses it affects also the sphincter pupillæ itself, whereas the dilatator remains for some longer time unaltered; the sympathetic nerve is not irritated by atropina. His view respecting the sympathetic he bases on the experience that in the whole animal frame there is no neuromuscular organ known which is irritated by atropina. I confess that by this hypothesis a great many observations respecting the movements of the iris can be explained, but I do not think it complete. I miss, first, all allusion to the trifacial nerve; it is thoroughly left out of the question. Second, there is no sufficient explanation of the most probable fact that atropina still enlarges the pupil after the trifacial and oculomotorius have been cut, and the pupil has taken a width corresponding to the paralytic state of these two nerves.

*c. Dr. Keuchel's Theory.** Dr. Keuchel considers the oculomotorius as an inhibitory nerve to the dilatator pupillæ, imagining that the ophthalmic ganglion sends nerve-fibres to the sphincter pnpillæ as well as to the dilatator, and that the oculomotorius, when irritated, paralyses the latter. It is true that by this assumption those phenomena we observe in cutting or irritating the oculomotorius, or applying atropina to the conjunctiva, can be explained. Cutting the oculomotorius, its inhibiting influence becomes eliminated, the dilating nerve-fibres† act alone, and enlargement of the pupil must be the effect. Atropina, in paralysing the oculomotorius, leads to the same result. The author finds his presumption most probable on the ground that, as has been mentioned above, curare does not paralyse either the sympathetic or the oculomoto-

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* L.c. pp. 74 and 75.

† The reader ought to keep in mind that these dilating fibres to which Dr. Keuchel is here alluding, are not the same which lie in the grand sympathetic. Such a presumption would be at once contradicted by the facts that in rabbits the sympathetic is not connected with the ciliary ganglion, and that we still get by an irritation of this nerve enlargement of the pupil, when the ciliary ganglion has been previously extirpated.

rius, whereas atropina paralyses only the oculomotorius as far as this nerve is concerned in the movements of the iris, while the sympathetic is not affected by the alkaloid. From the first fact the author concludes that the fibres of the oculomotorius do not go directly to the sphincter iridis; coming in this way to the assumption of a ganglionic centre. From the second he thinks he is compelled to establish a difference between the properties of the two nerves in question. As to the paralysis atropina produces in the course of the oculomotorius, Dr. Keuchel assumes that it is produced in this nerve itself, and not within the ganglion, to which it is running. This is the central point, respecting which Dr. Keuchel differs from Professor v. Bezold. I cannot find any fact or argument why I should prefer the one or the other of these theories; for if Dr. Keuchel says, that as section of the oculomotorius and paralysing it by atropina are of the same consequence, the alkaloid cannot affect a ganglionic centre; I respond, Why not? there is no necessity to assume that the fibres of the oculomotorius are connected with the same cells which supply the dilatator; the contracting and the dilating fibres can originate from different cells. This theory offers no complete insight into the mechanism of the movements of the iris. There is, first, nothing said about that central organ intercalated in the course of the sympathetic; for if Dr. Keuchel concludes, from the ocumolotorius not being attackable by curara, that this nerve must be connected with a centre, he ought to do the same respecting the sympathetic. Second, we find nothing about the manner in which the trifacial nerve is connected with the movements we study. Thirdly, it gives no explanation of the fact that after all the nerves have been cut it is still possible to enlarge the pupil by atropina. If further investigations should confirm this fact, then we shall be obliged to abandon Dr. Keuchel's theory; for in this case it is demonstrated that cutting the oculomotorius and paralysing it by atropina are not identical. Fourthly, it is thoroughly unjustifiable to devolve the duty of a centre on the ciliary ganglion and not also on the ganglia described within the iris and its adjacent parts.

From these objections it follows that there are still three points to investigate before we may boast of any clear conception of the movements of the iris, viz., first, to determine the influence of the trifacial nerve; second, to elucidate the true nature of the sympathetic; third, to try to make out if either the ciliary ganglion or the ganglia cells of the iris is the nerve centre, which we are obliged to suppose

exists in the course of the oculomotorius. At present I can only offer some insignificant remarks.

a. Influence of the Sympathetic Nerve.—About this nerve there are still two questions to be solved; first, if, while it traverses the skull, any dilating filaments enter into its course; and second, if it goes directly to the dilatator, or through any ganglionic centre, as the above-mentioned experiments with curara seem to indicate. The first question has been answered in the affirmative by Dr. Guttmann.* As I myself have not repeated the experiments of this author I cannot contradict them, but I do not think that physiology can look at this question as already set at rest. I have shown in rabbits that section of the sympathetic in the neck produces no contraction of the pupil; consequently, sympathetic fibres can be cut anywhere, and no contraction of the pupil ought to follow. On the other hand, when contraction after section follows, it is necessary to wait till we are quite certain that the contraction does not relax; for if this were the case, we should be obliged to infer that some irritation of the nerve had taken place, the nature of which it was to contract the pupil. If we do not, we have, as in the preceding case, to apply some stimulus, and to show that now enlargement takes place. Now I return to Dr. Guttmann's investigation. From experiments on frogs he concluded that, except the centrum cilio-spinale, detected by Professor Budge, from which the dilating fibres of the sympathetic, while on its course through the neck, are derived, there exists another centre in the ganglion Gasseri, from which dilating fibres of the sympathetic originate. But in not one of his experiments was the pupil actually dilated by irritating the sympathetic fibres themselves. He destroyed the Gasserian ganglion, and got contraction of the pupil stronger than when he cut the fibres running from the sympathetic into the ganglion. No reason is stated why the author did not say that contracting fibres take their origin within the ganglion he loosened from their centre, or that he had irritated some contracting fibres only passing through. It is true the contraction lasts rather long, but the section in a large ganglion may produce an irritation not so quickly disappearing as if we cut a simple nerve. Previous to these experiments Dr. Balogh,† of Pesth, had emitted the opinion that

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* De nervi trigemini dissectione apud ranam esculentam. Diss. inang. Beral. 1864, and Centralblatt für die Med. Wissenschaften, 1864, p. 598.

† Moleschott's Untersuchungen, Bd. viii. p. 423.

the trifacial nerve also contained fibres of dilating properties. He asserted that he had seen dilatation of the pupil when irritating the said nerve before entering Gasser's ganglion. Dr. Guttmann has denied the correctness of this experiment. I myself deny it too, but shall afterwards show that Dr. Guttmann is also in the wrong. As far as my experience respecting the sympathetic goes, there is until now not the slightest reason to believe that the sympathetic, while running through the carotid canal, receives still other dilating filaments than it has got up to this point. As to the other question, if the sympathetic be connected directly with the fibres of the dilating muscles or ganglionic cells, very little can be said. I have already mentioned the fact that in animals poisoned with curara the sympathetic does not so soon lose its influence on the dilator as voluntary nerves do; but I do not think this sufficient for assuming that there is a special ganglionic centre intercalated in its course, for of the behaviour of those nerves distributing themselves in muscles of non-striated fibres so little is known, that we had better wait till stronger proofs have been brought forward. When reflecting on this point it came into my mind that perhaps experiments of the following kind might be of some use.

Many years ago* I showed that if we suspend the nerves of striated muscular tissue in a strong solution of common salt, we do not get contraction of all the fibres at once, but in the beginning only some of them contract, then their number augments, and, as the contraction of all the fibres does not happen simultaneously, the muscle shows a kind of undulating motion.

I further showed† that if one applied the same kind of irritation to the vagi the motions of the heart are never affected in an analogous way. We observe only alterations in the number of pulsations in a given time. The heart beats slower and slower, and finally it comes to a stand-still in diastole. From this observation I concluded that the vagi were not the true motor nerves of the heart, for at that time we knew very little about the ganglia of the heart. To return to the sympathetic and the pupil, I argued: If the sympathetic ends in the dilatator pupillæ without the intervention of a ganglionic centre, it ought, when irritated by a solution of common salt, to produce no regular dilatation of the pupil, but irregular movements and distortion; for in this case the

* Henle's Zeitschrift, Neue Folge II. p. 303.

† Müller's Archiv. 1851, p. 205.

forces of the said nerve not acting simultaneously cannot be connected into a common aim; if, on the contrary, ganglia are intercalated, the irritation will produce an uniform dilatation in every diameter of the pupil. In testing this idea, most unfortunately I found that the sympathetic nerve gives no dilatation of the pupil at all. This observation is in accordance with that made by myself and Dr. Knoll when irritating, in the same way, the anterior corpora bigemina. Only in some few cases I got dilatation, and it was quite uniform; no irregular contractions could be observed. But I hesitate to acknowledge these few observations as being true and sufficient to prove the existence of ganglia within the channel of the sympathetic. To hasten to the conclusion we have finally to take in consideration—

β. *The Trifacial Nerve.*—As in this paper I do not intend to write the history of our knowledge about the movements of the iris, but have proposed to expose how far we may rely upon what has been said, I will only refer to the communications of later dates, and add my own experiments relating to this point. In the year 1855 Professor Budge published his treatise "Die Bewegungen der Iris," and declared himself a defender of the opinion that the trifacial nerve influences the movements of the iris, being contractor of it. In 1862 Professor Budge was opposed by Dr. Balogh,* who was of opinion, as I have already mentioned, that the trifacial nerve included fibres of dilating nature. Finally, in 1863 and 1864, Drs. Oehl* and Guttmann† asserted that the trifacial nerve carried neither dilating nor contracting fibres. Whom shall we trust most? It is true that these physiologists experimented on different animals, and it may be that this circumstance is partly the cause of the divergent opinions; but it seems to me that also a certain ease in explaining the phenomena observed has a share in it. I myself have made experiments on rabbits, and will now report what I have seen and how I interpret the facts observed. In every experiment I began with dilating the pupil by atropina. By so doing I intended to paralyse the common motor of the eye, in order to avoid any deception into which I might be led when operating near this nerve. Then I irritated the trifacial electri-

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* L.c.

† Della influenza che il quinto paio cerebrale dispiega sulla pupilla. Firenze, 1863.

‡ L.c.

cally before entering Gasser's ganglion after having excerebrated the animal and cut off the head, or I dissected the nerve on the living animal. In no case have I seen dilatation of the pupil; but I have constantly observed contraction of it. As respects the electrical irritation I am obliged to remark that it ought to be done rather quickly after having taken away the cerebellum, for the nerve very soon loses its vital properties at its cerebral extremity. Even when cutting away the cerebellum, and arranging the nerve for its irritation, we meet decided contraction of the pupil. In every instance the contraction lasts for a certain time. When I experimented on the head cut off, the contraction lasted so long that it seemed as if in this state the rigor mortis had set in; but when I brought the pupil to contraction in the living animal by cutting the trifacial, the pupil after a certain time took a width nearly equal to that I produced by cutting the optic nerve. Let us now analyse these observations. What is the meaning of the fact that you get contraction, but not persistent, when you cut the trifacial, say in the ganglion Gasseri? We are forbidden to refer this phenomenon to the oculomotorius, as this nerve was previously paralysed by atropina. To explain it by the assumption that dilating fibres had been cut, and contracting ones had come to preponderate, is opposed by the observation that the pupil in the living animal dilates itself afterwards. The sympathetic of the neck too contains undoubtedly a large amount of dilating fibres, as is shown by its irritation; as I have already mentioned, no contraction is observed after its simple cutting. Consequently we are constrained to believe that the phenomenon in question is produced by irritation of contracting fibres. Where do they come from? Of course, either from Gasser's ganglion or from the trifacial; but as we are able to contract the pupil by irritating the trifacial immediately after its exit from the brain, I see no reason why I shall not assume that the trifacial nerve contains fibres going to the sphincter pupillæ. I do not believe that there is any fact observed by the above-mentioned authors which contradicts this view. As to Dr. Balogh's* assertion that irritation of the trifacial before entering the ganglion gives dilatation of the pupil, I consider it erroneous. Very likely parts of the current went through filaments of the sympathetic; I believe the more, that this has been the case, as in my own experiments I observed a certain degree of contraction while excerebrating the

* L.c. p. 433.

animal and severing the trifacial from the brain. Neither can I accept the explanation Professor Oehl* offers respecting the fact so incommodious to him, that in rabbits one gets contraction of the pupil when opening the skull. He is of opinion that, while opening the skull, the first branch of the trifacial, including the sympathetic fibres, is so much irritated that it becomes paralysed, and, consequently, the pupil contracts. But I think that sound conceptions of the physiology of the nerves can never lead to such ideas. First, I ask where shall take place irritation of the first branch of the trifacial to such a degree that paralysis is the consequence? For according to Professor Oehl the dilating fibres in question originate within Gasser's ganglion, whereas, in the abovementioned experiment, we only take away the cerebellum so cautiously that we get a long stump of the trifacial, and do not at all touch upon the said ganglion. Second, I have no idea how nerves can be paralysed by invigoration, not having been before irritated. But such an irritation has not taken place, for in this case the pupil would have become enlarged previous to its contraction. But this is by no means the case. The fact that by irritating Gasser's ganglion or the first branch of the trifacial nerve one gets dilatation of the pupil proves nothing against my view; for as the trifacial loses its vital properties sooner than the sympathetic, it is obvious that the irritation produces dilatation. I believe that at present it cannot be doubted any longer that the trifacial is contractor of the pupil, the only point still to elucidate is to make out in which way the said nerve acts. As at present I have no clear idea thereon, I refer the reader to future researches.†

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* L.c. p. 25.

† Meanwhile see Grünhagen: Berliner Klinische Wochenschrift, 1865, p. 253.

OCCASIONAL PAPERS.

By B. W. Richardson, M.A., M.D., F.R.S.,
President.

EFFECTS OF SEASONAL CHANGES ON THE ANIMAL BODY.

Thirteen years ago my friend, the late Mr. Milner, of Wakefield, related a series of observations, of first importance, in evidence of the fact that the whole nutrition of the body undergoes periodical fluctuations in weight, substance, and form, under the influence of seasonal changes. From his position as surgeon to the convict establishment at Wakefield, Mr. Milner was enabled to weigh every prisoner at given periods, to compare the gain and loss throughout the year, and to estimate such gain and such loss, by individual against individual, and by diet against weight. The prisoners upon whom the observations were made had been sent to Wakefield to undergo the first portion of their punishment. They were kept in separate cells for a period of nine months; they were all males, between the ages of fifteen and sixty, and they were all in good health when they arrived. Their cells having an equal capacity and the same means of ventilation, they were served with the same quantity and character of air; and the mean temperature of the cells was in every case 61°. The men were all fed on the same kinds of food, they were all dressed in the same attire, and they were made to take the same amount of exercise. They were weighed on admission, and again at the latter end of every calendar month during their stay. The number of men weighed by Mr. Milner exceeded 4,000; the period of time occupied in his observations was ten years; the average number of prisoners weighed monthly was 372; and the total number of individual weighings was 44,004.

I have given these details with care, because they refer to an experiment so great in its design and completeness, that it cannot be over-estimated. Here were men living, year after year, under the same conditions day by day, eating the same food, breathing the same equable air, and doing regularly the same amount of physical work. One would predicate that, under these circumstances, the nutrition of the body would remain nearly the same; but the prediction would be entirely wrong, for there are periods, specially marked out, when the body is gaining in weight, and other periods, as specially marked out, when it is decreasing, and these, year after year, as if by a general law.

To begin then with the first months of the year, we learn that the body undergoes an average loss of weight in January, February, and March, the proportion of loss being 0·14 in January, 0.24 in February, and 0·95 in March. During the months of April, May, June, July, and August, there is gain in the following proportions: —for April, 0·03; for May, 0·01; for June, 0·52; for July, 0·08; for August, 0·70. In September, October, and December, there is loss, in the proportion of 0·21 for September, 0·10 for October, and 0·03 for December. November presents an exception to the months that precede and follow it, there being an average gain of 0·004; so that, at first sight, November would seem to be an exception in the losing series of months, in a very, very slight degree. But Mr. Milner points out that this apparent exception was caused, in the prisoners, by the arrival of large numbers of new men in each year, and from the fact that the men usually gain weight for a short time after they are received; so that this break, in the series, results from the influence of the stage of imprisonment. On the whole there is an average loss beginning in December, and increasing rapidly up to March. In April there is an abrupt gain, which extends irregularly till August. In September there is a rapid loss, which continues, but not to the same extent, through October. From these facts Mr. Milner draws the following inferences:—1. The body becomes heavier during the summer months, and the gain varies in an increasing ratio; 2. The body becomes lighter during the winter months, and the loss varies in an increasing ratio; 3. The changes from gain to loss, and the reverse, are abrupt, and take place about the end of March and the beginning of September.

For clearness of illustration we may put the facts into two tables of loss and gain, keeping the months distinct.

MONTHS ATTENDED WITH LOSS OF WEIGHT.

Month.	Proportion of Loss.
September	0·21
October	0·10
(November ?)	
December	0·03
January	0·14
February	0·24
March	0·95

MONTHS ATTENDED WITH GAIN OF WEIGHT.

Month.	Proportion of Gain.
April	0·03
May	0·01
June	0·52
July	0·08
August	0·70

In these observations, which may be accepted as conclusive, we gather a series of facts, which, at first sight, would appear to be different from common experience; they are, however, in entire accord with such experience when it is collected with the same care and the same attention to details. What is more, they are in accordance with physiological truth; for Dr. Edward Smith, in his most valuable experimental inquiries, has shown that the quantity of carbonic acid exhaled from the body is largely increased in winter and is decreased in summer. He has further indicated, in an independent manner, that there is a sudden change in regard to the evolution of carbonic acid in the months of March and April; the quantity of carbonic acid thrown off in March being very much greater than in April. Thus, his observations are in entire accordance with those of Mr. Milner, inasmuch as evolution of carbonic acid in increased quantities implies waste, and in diminished quantities gain.

I have introduced the above physiological argument into this essay, not simply for the purpose of showing the influence exerted by atmospheric conditions on the body in health, but to indicate how remarkably the variations that have been described tally with certain diseases. We know that the chief outlet for carbonic acid is the lung, and it is fair to infer that, if the physiological statements are to be trusted, certain pathological sequences ought to follow; in other words, at those times when the body is

performing the most active functions any interference with those functions ought to be marked by the most vigorous demonstrations of disease in the organs concerned. Is it so?

During a portion of the time that Mr. Milner and Dr. Smith were carrying out their inquiries, I was myself engaged in investigating the rate of mortality from certain diseases during special months of the year. I took the returns published by the Registrar-General, and analysed the causes of 139,318 deaths, occurring during the years 1838-1853, from the following diseases:—small-pox, measles, scarlet fever, whooping cough, croup, diarrhœa, dysentery, cholera, influenza, ague, remittent fever, typhus, erysipelas, quinsy, bronchitis, jaundice, carbuncle, and pneumonia. I selected these diseases especially, because they are less liable to error in the returns, they being diseases easily recognised. Some of these are disorders which are directly connected with atmospheric variation; others are only indirectly connected. Let us see how the facts stand in respect to the disorders affecting the lungs. I name bronchitis first. Turning, then, to the months in which this disease is most prevalent, I found that the deaths from bronchitis attained their maximum during the three months of January, February, and March; that in April, May, and June, they largely declined; that the declination continued during July, August, and September, beginning, however, to ascend towards the close of September; and that they began steadily to rise in October, November, and December. The per-centage runs as follows:—In January, February, and March, 36·793; in April, May, and June, 20·301; in July, August, and September, 18·327; and in October, November, and December, 32·570.

Croup stands precisely in the same category as bronchitis. Thus, from this disease the per-centage of deaths in the months of January, February, and March, was 27·523; in April, May, and June, 25·100; in July, August, and September, 19·919; and in October, November, and December, 27·456.

Pneumonia afforded a series of observations running parallel with those that had gone before. Thus, in the months of January, February, and March, the per-centage of deaths averaged 37·022; in April, May, and June, 19·631; in July, August, and September, 12·324; and in October, November, and December, 33·521.

In regard to catarrh the same careful series of observations could not be made, inasmuch as no returns of mortality under the head catarrh are supplied, cases beginning as catarrh and ending

fatally being, I may say, invariably classed under the heads of bronchitis, pneumonia, croup, and influenza. But experience tells us that whenever the more formidable diseases are most prevalent, the number of cases of catarrh are also most prevalent; so that this disease, which is indeed but premonitory of the severer types of affection of the lungs, or is a modified form of them, may be classified in regard to its periods in the same manner.

The same observations apply to pleurisy.

One other disease in which the lung structure is involved, I mean influenza, shows a certain variation from the preceding diseases in point of periodicity; that is to say, the mortality from it attains its maximum in the last three months instead of the first three months of the year; but in other respects the analogy is complete. Thus, the per-centage of deaths from influenza is rendered as follows:—In January, February, and March, 23·539; in April, May, and June, 12·171; in July, August, and September, 4·502; and in October, November, and December, 59·784.

From the data supplied to us by Mr. Milner we may gather some useful lessons respecting the variations of the body in chronic diseases. Thus it is of importance to remember, in treating phthisis and other exhaustive disorders, how great will be the natural waste of the body in some months, especially in March, February, September, and January, and how marked the natural gain in other months, as in August and in June. Lastly, in regard to clothing and alimentation, we may be able to convey the most valuable instruction to the sick, if we remember the particular seasons when waste or increase are in the natural order of the phenomena of living action.

ON THE USES OF NITROGEN IN ATMOSPHERIC AIR.

Experiments which I and others have elsewhere related show clearly that an animal will die if it be placed in pure oxygen; but it is worthy of remark that while oxygen in the undiluted form stops life, because it cannot enter into combination at temperatures below 50° Fahrenheit, the same oxygen unadmixed with any other gas, if distributed and brought into active motion *by heat*, will destroy ultimately, by just the opposite process, that is to say, by setting up too active a combustion. Thus if our atmosphere were composed entirely of oxygen, and if the same character of seasons could prevail, and the same variations of heat and cold could prevail, all life would be impossible. At the poles the oxygen would be so condensed that combustion of the blood would be prevented. At the equator the combustion would be so rapid that according to our present conformation we could not receive sufficient food to sustain the combustion. In the temperate zones, when the thermometer was below 40° we should die as from want of air; and when the temperature was above 80° we should die as from want of food.

All these points admit of demonstration, the time being given to show animals living in pure oxygen gas at different temperatures. I cannot do this on the present occasion, but I have named the general facts in order to point out what I think is a new truth in respect to the uses of nitrogen to man. It is common to speak of nitrogen as the mere mechanical diluent of oxygen in the air. I understand it as more than this. It is the grand equaliser of heat. Thus, when the day is cold the loss of heat from the atmosphere is sustained as four to one by the negative nitrogen; and thus, when the day is hot the increase of heat in the atmosphere is sustained as four to one by the negative nitrogen. Thus the oxygen, although it undergoes physical change under great variations of heat and cold, although it is actually more condensed at the poles than at the equator, is never so much disturbed but that it can sustain a certain degree of life.

By the presence of nitrogen, equalizing the distribution of heat over the whole surface of the earth, the fluctuations in the activity of oxygen on man are kept within given limits. But there are

fluctuations, notwithstanding, sufficient to lead to definite physiological phenomena affecting and influencing, not individuals only, but races of men, and by virtue of which the body is enabled to live in various parts of the earth under the most variable conditions.

If I take an animal from a temperature of 60°, and place it, not in cold oxygen, but in cold common air, at 30°; if I feed it well and cover its body closely; if, in fact, I place it in the condition of a well-fed Esquimaux, I find that the animal will want to eat largely, begin to make an excess of carbonic acid, and if only fed as at 60°, will commence to waste. The reason for this series of events is that oxygen is abundant in the air, and at the same time is sufficiently diluted to be able to combine with the blood; and the result is a greater production of primary force by which the animal is enabled, when well fed, to sustain the effects of the surrounding cold.

If from this extreme degree of cold I move the animal to a temperature, say of 70°, still supplying it with common air, I find, if the food be kept up, and all else be equal, the animal ceases to crave so much for food, produces less carbonic acid, and with a decreased waste tends to grow fat. The reason for this series of events is that the oxygen, diluted still for ready combination, does not meet the blood with the same degree of pressure; and the result is that the animal, which in the warmer medium does not require so free a production of force, produces less force.

But if, in repeating these experiments, I use pure oxygen instead of common air, the animal at the lower temperature will want no food, will make a minimum of carbonic acid, and will die from hot burning; while the animal in the higher temperature will eat ravenously, get very hot, produce excess of carbonic acid, and, if not over-sustained with food, will die from waste. The differences in the results of these experiments, as compared with those related before, are due to the absence of the equalizing nitrogen, which, existing in the proportion of four to one in common air, resists, in that proportion, the excessive action both of heat and of cold.

ON AN ADAPTATION OF THE REFLECTING MIRROR TO THE UTERINE SPECULUM.

By Leonard W. Sedgwick, M.D., Honorary Secretary of the Association.

To see better is so frequently to know more, that I do not hesitate to submit for the consideration of the members of the Association, an adaptation of the reflecting mirror to the uterine speculum, which for some time back I have found most useful.

The mirror is three and a half inches in diameter, and is of five inches focal length. Like the laryngoscope mirror it is mounted in a light metallic back, and to its circumference a brass arm is attached by a ball and socket joint, which is easily tightened or slackened by a screw movement. The arm can be lengthened or shortened by a simple telescopic slide; it is bent at a somewhat obtuse angle close to the joint, and is, when closed, four inches long. At its other extremity the arm is also bent at an angle, and is square; it fits into either side of a short socket attached to the upper surface of the outer end of the speculum, and is speedily secured there by a turn or two of a screw cap.

By this means the mirror has a firm easily-fixed attachment to the speculum, and by means of the ball and socket joint, and the telescopic movement, it can be placed at any angle in relation to the axis of the speculum, and at such a distance from it as may be most convenient or may afford the best illumination.

The mirror may, of course, be adapted to any speculum. It has been made for me, with their usual success, by Messrs. Weiss and Son, and I have latterly used it attached to a speculum of their construction, which, I venture to think, is the most satisfactory instrument yet made.

The form of the speculum originated, I believe, in a suggestion of Sir James Simpson. The lower blade is of the duck-bill shape, the upper is flat and fits on to the lower in such a manner that a

small interval is left between the two, so that in closing the instrument for withdrawal no pinching of the vaginal mucous membrane can occur. The upper blade is shorter than the lower one, so that when the speculum is opened, the cervix is easily received into it. The duck-bill shape of the speculum secures the very easy introduction of the instrument, and also so distends the vaginal walls as to prevent them falling in and obscuring the view of the os uteri. By this configuration the wooden plug is rendered unnecessary, and lateral blades are made useless; both very desirable arrangements, for by the consequent simplicity of construction thorough cleansing of the instrument is facilitated, and no absorbing surfaces or tortuous chinks are left where infecting particles may lurk or be overlooked. The dilating mechanism is Messrs. Weiss's improvement of Cusco's. A reference to the accompanying Plate II. will make this description intelligible.

By the use of the mirror thus affixed it becomes unnecessary to alter the position of the couch or bed on which the patient is lying, for wherever the source of light may be, the mirror can be so adjusted as to reflect it upon the cervix uteri.

There are no formidable preparations to be made, for the patient may lie in the usual obstetric position in the bed wherever it may be placed.

A better illumination is obtained by daylight, inasmuch as a larger amount of light-rays are concentrated on the spot to be examined than can be obtained by direct transmission.

A better illumination is also obtained when artificial light is used, for not only does the same reflection of light occur as in daylight, but the part is relatively as well as positively brighter, for as the lamp or candle may be placed on the table at some distance away, the great disadvantage of looking over or through a bright light at a place necessarily not so bright is avoided.

The impossibility of the occurrence of such unpleasantnesses as the dropping of grease, or the singeing of bedclothes, is an additional recommendation.

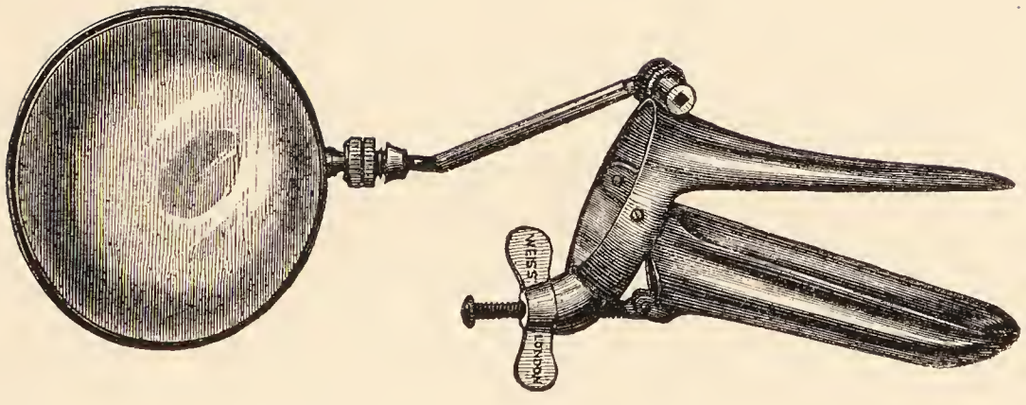
In all cases one hand of the medical man is entirely set at liberty, and in many instances both are free. Where the speculum does not of itself retain its position, and both hands are needed, the finger of an assistant or of the patient herself serves to secure its steadiness.

If the mirror be on a lower level than the source of light, and this may always be managed, it does not interfere with the use of

both eyes; and, as it is always set at an angle, it is never in the way of any instrument which may be in use. One can thus accomplish with greater certainty many necessary mechanical interferences with the os uteri, because both hands are at liberty, and one has a clear and continuous view of all that is going on, especially if the instruments used are, as they may often be, trowel-handled.

I may add that this mirror may be used attached in a similar manner to the rectum speculum, with advantage.

PLATE II.



DR. SEDGWICK'S ADAPTATION OF THE REFLECTING MIRROR TO THE UTERINE SPECULUM.

III.

COMMUNICATIONS ON GENERAL AND SOCIAL SUBJECTS.

ON THE BEST MEANS OF LESSENING CRIME.

By George Cordwent, M.D., F.R.C.S. Eng.

To lessen crime, you must lessen temptation or strengthen resistance. To lessen temptation seems impossible, because it is Protean, and ever varying with the varying phases of human society and of individual man's condition—its elements are ever playing about humanity, and are man's allotted probation. I would separate political crime from moral crime, because human laws being fallible, political crime may be, as it were, *natural evidence*, and so awaken attention; just as dire outbursts of cholera induced first sanitary philosophy, then sanitary measures; or as repeated smuggling, unchecked by punishments, brought slowly free-trade; but as political crime cannot in the present state of our country be palliated by any theory of unreasonable obstruction, resistance to law becomes moral crime when pressure external is listened to by our government, and sad it must be for any country in which fervid legislators experiment on anticipated and undeclared wants. The daily records of moral crime prove that this has a *domestic* and family growth; that its aptitude grows with growth, and strengthens with strength; that once thoroughly imbibed it is not deterred by punishment, but is in fact more an inherent taste than induced by temptation. I am not, of course, arguing that crime is never induced by temptation—we see the contrary—we see in times of dear food our gaols filled for petty larcenies; not that men all at once are more immoral, but more hungry, and their morale insufficient to resist; we see, too, our game shops tempting the poacher. I assume the acknowledged fact that temptation must always have its influence, and that in rare instances it may be even irresistible; and that as malaria is to the body, so is temptation to the mind—a force to conquer or be conquered; but, in the great majority of cases, crime is committed under what ought to be very

slight, or under even no appreciable temptation. It is against this we ought reasonably and solicitously to build, and I believe the women of the nation must chiefly do this great work, a work far nobler than could be allotted them by political franchise or other contended rights—to form the gentle and affectionate brother and sister, and, through them, the future friend, husband, citizen, patriot, law-giver, and all, to do their duty—seems allotted by the Divine hand to woman, whose weakness and whose excellences are not less solemnly intrusted to the care of man. Let women then proudly think of this, and believe that a nation will be great or degenerate, not in proportion to its learning and wealth, but in proportion to its healthy strength and goodness; let them believe that with few, extremely few exceptions, children inherit good constitutions and bright intellects, in proportion as parents are temperate and industrious—that they will be good, and afterwards good citizens, in proportion as they see practised and hear inculculated truth and uprightness—brothers and sisters will be kind to one another, as fathers and mothers set an example of kindness and politeness.

In early childhood selfishness is strong and conscience nugatory, so that habits of dishonesty are then most easily adopted, and if not controverted by watchful care before the age of eight years, I believe the tendency becomes confirmed; but fortunately with the instinctive art and selfishness of early childhood, there is a pliability of mind over which, if crime is to be lessened, the women of our country must watch with most solicitous care, for soon from this pliant stage in youthful life another critical one with wider range opens to the outer world, and with no medium tendency *sentiment* helps the moral conscience now first springing into life, or a perverted ambition confirms a natural depravity. Now, too, when comes an anticipated withdrawal from home supervision, what so preservative as loving remembrances? let us then minister to this piety of family affection, by, as far as possible, making the amusement of each family complete at home; by making home, in short, the most pleasurable resort. Now, suppose a family brought up under such favourable and reasonable home influence, let us ask ourselves, and bring experience to bear on the answer, will the young men be likely to abuse their relationship to women, and so spoliate a natural, a reliant love, so sublime and devotional as to be worthy God's gift to human society as its best and most fostering endowment? will the sisters of such brothers, the daughters of such mothers, become in their turn good wives and

mothers, re-instilling all the good impressions they received? Let any man answer.

Women with more quickness, but less scope of mind than men, have instincts more *direct* and more domestic, so Divine provision made the leaning of these instincts more pure, to conserve the best ordinances of society; let us be careful how we pervert them, for out of them are the issues of social life; they are the modellers of impressionable human clay, and the vessel will "be formed to honour or dishonour" according to the touch they bestow; so when the child clings to the mother, let it be well it should do so —let it be well that she must guide its tottering wavering steps till these become determinate; the child too, by its essentially imitative nature, is led both by love and ambition to copy his parents as his highest imaginable authority; both father and mother, then, by every observed action contribute to the formation of character in the child, but the mother in by far the larger share, as nature and necessity bring the young child much more under her immediate influence. To lessen crime, then, let women be impressed with the greatness of their power, the sacredness of their trust; and let man help them in this, by preserving with care their instinctive tendencies—men, even very young men, are easily brought to see the justness of this and its inexpressible advantage to society, and, except in rare instances, the feeling will be spontaneous in those who have habitually reciprocated kindness and affection with their brothers and sisters. If then from the garden of humanity vice is in any important degree to be weeded, it must be done in the germ, and only the quick perception and adroit hand of woman can do it—later you may scarify and tear, but you must leave the fibre; tell women this, and not one in a thousand, though ever so fallible in other respects, but to whose heart this maternal truth will go direct with the force of a solemn instinct. You cannot lessen ever-varying temptation more than you can lessen the elements that play about the earth: you can deter crime by fear, that we see every day, but you can strengthen resistance by early impressing the heart and affections; neglect this golden, this *natural* opportunity of early childhood, and allow its instinctive art and selfishness to go uninfluenced, and the future character is irretrievably deformed; lose this opportunity which *instinctive nature* forcibly attracts us to, and society, whilst punishing the vicious victims by judicial horrors from which the sickening eye turns away, expiates its own greater offence, by enormous expendi-

tures, and by suffering incorrigible trespass and insecurity. But as in disease, so in social evils, the ultimate tendency is alleviation or cure, indirect no doubt, and each in its own way; still even evil is economised, and crime, the great ulcer in civilised life, like other diseases has a teaching influence, and now having become painfully severe, we begin to hunt out its pathology, and doubtless we shall find it secondary and remediable.

The theory herein proposed may be thought visionary, but I am convinced it is true to nature; and that women are not sufficiently impressed how the good, the great, and the graceful, are nearly always those who have had most affectionate care bestowed on them in early childhood; and how can it be thought women are impressed that in them lies the influence which forms the spirit and morale of each rising generation, when large numbers of them are seeking the poorer ambition of admission to the forum? or how can it be thought men are sufficiently imbued with a feeling of the vast, the surpassing influence of women on the social order of the country, when at public meetings they invoke them only when wine has passed free, instead of, in the order of toasts, placing the mothers of our children next the altars of our religion? Many place reliance on education as a corrective to crime, but how can it be so, when the intellect *obeys*, not governs, the passions? education stands only in the position of an instrument—of a servant to good or evil, according to the dominant will. Then of religion—we see that organised religion does not diminish proportionate crime; or why does social science invoke another force against its multifarious increase? That the great spirit of religion has its root deep in the nature of man, deep as love, I am fully convinced, and to be without it is a hideous deformity of character; so by natural evidence alone it is clearly ordained by the Creator to play a great part in human life; let then its divine afflatus be breathed early on the child, it will temper its unfolding soul. But, in the later phases of man's varying life, that religion may bring aid at all times, and consolation often times, would it not be better that sermons should habitually, instead of rarely, teach that good or evil, inevitable, if not immediate in this human life, is the fruit of conduct, and that conduct is the fruit of the heart? Religion so taught would guard the good seed in its growth; but long observation and thought on such subjects convince me, that growth, to be strong, must first be well and carefully fostered by moral influences, and so early that only a mother's care can bestow them.

Another particular of utmost importance to its preservation is the state and accommodation of the dwelling; "things outward do draw an inward quality after them;" how then must dingy hovels affect the temperament and the habits! Hovels are, in fact, the curse of the operative classes, and perpetuate a poverty not justified by their wages; but hovels destroy a pride in home, which, if you once make pleasing to the eye, industry will soon make comfortable, and honourable care more fully endow; if, on the other hand, a faulty accommodation assault decency, she soon flies away, and takes love in her departure; where then is human dignity? where then the pride of character which, more than pure conscience, holds us in keeping? A miserable defective dwelling endangers what would otherwise be noble, confirms the less fortunate, and adds a most ungenerous and unjust weight to the scale of temptation. Moral and even political leanings are much more associated, even in adult life, with the material structures of home than is commonly imagined, and if it be true that outward impressions have a deep-set inward influence, this should be especially acted on in bringing up the young, and it is so acted on by the more wealthy—the squire has works of art and flowering gardens; the rural cottager, too, is helped by nature, his children are ever by force of instinct roaming in flowery fields and seeing the grand loveliness of nature; but the poor alley man of our towns, his unfortunate children! let me invoke consideration for them: what compensation have their infantine minds—what healthy impress can they get when straying from their dwelling—from the dwelling where a flower could not live—where a rose must be hectic, and "hearts-ease" die in a day? But if the human offspring can be presumed beyond the law that makes even the elements the vassals of intelligent will, and that the child is thrust at birth helplessly on a strong parental bias, let our penal code in mercy be altered. On the other hand, if, as most men think, *circumstances* vastly modify, and can *always* bring beyond the pale of evil the natural inclination, let the country look to it, and by organised supervision of our poorer children's homes, give each generation a fair chance and herself a fair future. Again, in training a child you are dealing with organic law, and with moral law through the medium of organic law, and when we daily see plants, which grown in sunshine and pure air, innocent and sustaining of human strength, but that the *same*, grown under opposite and deteriorating influences become deadly weeds, we see only one of the analogies to social

life; let us devise then, less of tortures to deter, than of modes to prevent by rational training; it would cost the country less, immensely less—for crime "costs us now, in one way and another, about as much as our poor rate, half as much as our navy, and probably nearly as much as all our civil services together." Instead then of spending several millions annually in detection and punishment of crime, would it not be far better and more just, to spend a few hundred thousands in *wholesome* rearing by the state, of all children deprived of suitable home influence, and punishing or taxing the parents. In the Horton-Kirby "Home for Little Boys," we have an example of what rational training can do for children taken young from the same class as those usually brought up in union workhouses; and whilst from the "Home" scarcely an instance occurs of a boy turning out badly, from union workhouses, where all is dull, cold, and unaffectionate uniformity, more than eighty per cent. turn out badly; they leave their dull abode, "stupid, greedy, and entirely without affection;" and we see that this is not because nature could not easily yield a better produce, but because the cultivation is utterly unskilful, and is it likely to be otherwise with a system devised near forty years since, and controlled by commissioners sublimely distant, and by parish guardians watchfully *near?* Some attribute crime to bad paternity, and in this theory are bold enough to suggest a cruel and most dangerous violence to the strongest fibre that runs through human life; but if paternity give inflexibility, why are children of the same class from one institution sprightly, grateful, and affectionate, and just the reverse from another, smitten down as it were by catching the dull uniformity of its crude convenience? Then again, this argument of paternal cause is negatived by the later history of our penal settlements—the children of convicts, born and brought up in these settlements, under the same circumstances as children of other settlers, are said to be quite as amiable and trustworthy. I do not doubt, however, that disposition like disease, but less determinately, is hereditary; but then it is only a disposition, or a leaning, which circumstances can and do control; and if we see the young of even ferocious animals made amenable by human care, and the sagacious and friendly dog becoming petulant and treacherous under unfair treatment, can we still believe in the intractability of parental leaning—still believe the Divine hand has left him a helpless victim—him, the only uninfluencable, whose intricate future implies

most necessity for guidance, and made his life to dawn upon a dark future of inevitable degradation and wretchedness? If we do believe this, then alas for human justice! call it criminal justice! and let our consolation be, that somehow, somewhere, the eternal balance shall be struck, and that moral life, like physical life, can be only *part* of a system; but, instead of uncontrollable hereditary disposition, the more reasonable and partially applied belief is, that the human mind at all ages, but especially in childhood, is conditionable to training. I believe it then entirely so in its moral relation, and more diversely so in its intellect; but even in adult life, continued strong circumstances conditionate the mind and alter the visage, just as physical shape even then slowly yields to habitual posture; and accepting this belief, as the public generally do, let us establish throughout the country large nurseries, whose internal and external conditions shall be quite unlike, but not necessarily more expensive than the crude ones now in vogue, and to them let the authority of law remove children from every home which drunkenness, dishonesty, or squalor, has made unfit, and rigidly tax the parent in contribution; and, should he fail to pay, let him lose the rights of citizenship, and be otherwise promptly punished—let the State assume parentage, when nature has alienated it; and if we people too fast for the home resources of our small island, there is plenty of cheap room in a salubrious and easily reached colony, where thousands of uncultivated square miles reprovingly await the delayed hand of industry—let us have schools there, affiliated to earlier ones at home, and in this way the evils and discomforts springing from the crowded and untrained portions of our population, may turn to good a never-failing necessity, which in some form or another, like a law, dimly guides the course and fate of nations: whilst children are brought up as now, in union workhouses and city hovels, and whilst waifs are baby-farmed, is it a thing of wonderment that our country should be overrun with crime, and our gaols full; and that the unhappy wretches in their turn should bring retributive justice, by enormous burdens upon the State, by insecurity and police espionage? If it be true, almost without exception, that "as the child so is the man," reform ought to begin where the mass of those pass their first years of childhood, who, in after life, are known in our courts of law and gaols as "incorrigibles." To this end let us devise better national nurseries, wherein the natural impulse and instinct may be *individually* watched and guided, not smothered nor

perverted. And let us appeal to woman in this great work—her sympathies are always maternal, though not always called on to exercise them towards her own offspring; let her extend her pitying hand to children beyond the pale, and, till better institutions are formed, to children in union workhouses, and do even there, by a sound and legalised organization, what in her lies, to check these fertile crops of vice and after-wretchedness which present conditions are ever necessarily imparting to the population; and when she succeeds, as I am sure she can and none else, if only partially, in this regenerative blessedness, let the nation honour her, and say "Thy gentleness hath made me great."

IV.

APPENDIX.

NOTE.

The following Memorandum on the restriction of medical degrees to ten annually, was submitted to the General Session held on April 8th, 1869, and was received and adopted. It has been thought right to insert it here, in order that the Members may be fully informed on a subject which the new Assessor, the President, will doubtless bring before the University Court at an early period.

April 30th, 1869.

MEMORANDUM ON THE REGULATIONS CONCERNING THE CONFERRING OF MEDICAL DEGREES BY THE UNIVERSITY OF ST. ANDREWS ON PERSONS WHO HAVE NOT KEPT TERMS AT A UNIVERSITY.

For a long series of years the University of St. Andrews was empowered to confer the Degree of Doctor of Medicine, after examination, on practitioners who had not received their education there.

The Universities (Scotland) Act appointed certain Commissioners to revise the regulations generally of the Scotch Universities; and they were instructed to "provide that, in so far as shall be practicable, " and, in the opinion of the Commissioners, conducive to the well- " being of the Universities, and to the advancement of learning, " the course of study, the manner of examination, and the con- " ditions under which degrees are to be conferred, shall be uniform " in all the Universities of Scotland."

In accordance with these powers the Commissioners issued certain ordinances regulating among other matters the conferring of Medical Degrees, and they explained in a Report to Her Majesty the Queen the nature and effect of these ordinances, and of the provisions of the Act itself.

In their Report the Commissioners say that "the peculiar " circumstances of the University of St. Andrews afforded grounds, " in our opinion, for not insisting on absolute uniformity in every " respect." And so they did not require " in St. Andrews, as in " the other Universities, that every candidate should have passed " one of his four years of study in the University itself." But they " required in all ordinary cases that two of the four years of study " should have been spent in a University."

In reference to the ancient power of conferring Medical Degrees on non-resident candidates after examination, they say:—"It was, "however, represented to us, on the part of the Senatus Academicus, "that instances sometimes occurred in which medical practitioners, "who had commenced the study of their profession without the "intention of proceeding to a University Degree, were desirous, "after a successful career of some years, of attaining a higher "professional position; and that the absence of any restriction as "to the schools, in which candidates for the St. Andrews degree "might have received their education, had hitherto enabled such "persons to accomplish that object. The names of gentlemen of "eminence in their profession, who had graduated in St. Andrews, "were mentioned to us, and we ascertained, that in the medical "profession a general opinion prevails, that such a facility, as "that which the University has been in use to afford, is proper "and advantageous if confined within due limits. Proceeding "on this opinion, and after making the most diligent enquiries "as to the number of persons on whom it might probably be desirable "to confer a degree under such exceptional circumstances, we resolved "to empower the University of St. Andrews to grant the degree of "Doctor of Medicine to registered medical practitioners of established "position, without restriction as to the schools at which they had "received their education, on their satisfying the Medical Examiners "of the sufficiency of their professional knowledge; but, at the "same time, we thought it necessary, with the view of preventing "any abuse of the privilege, to limit to ten the number of degrees "which should be so conferred in any one year. That number, "we were assured on the best professional authority, would be quite "sufficient to include all persons so situated, who could present "any reasonable claim for admission to a degree."

The Regulation thus referred to, Section XX. of Ordinance No. 19, is as follows:—"The Degree of Doctor of Medicine may be con-"ferred by the University of St. Andrews on any registered Medical "Practitioner above the age of forty years, whose professional "position and experience are such as, in the estimation of the "University, to entitle him to that degree, and who shall, on "examination, satisfy the Medical Examiners of the sufficiency "of his professional knowledge: Provided always, that degrees "shall not be conferred under this section to a greater number "than ten in any one year."

The Commissioners foresaw that the regulations they framed

would very greatly diminish the number of degrees conferred by the University of St. Andrews, and they looked forward to such a result with no regret in the case of a University possessing no Medical School. In their Report of December 20, 1861, they say, "It did not belong to the Commissioners to entertain any "question as to the expediency of the University of St. Andrews "possessing the power of granting Medical Degrees, as the Com- "missioners were not conferring that power on the University, "and had no authority to take it away. But it appeared to "them to be their duty to consider in what manner the system "might be regulated, so as to insure, as far as possible, that the "power which the University undoubtedly possesses should be "exercised with due regard to the credit of the University and the "interests of the public." And in their final general Report to Her Majesty they add:—"The principal object, which we pro- "posed to ourselves in the Ordinance, was to insure that no "degrees should be conferred, except on persons possessed of suffi- "ciently high qualifications."

On an examination of the place of education of the candidates "for the degree of M.D., they find that they are almost entirely "persons educated and resident in England or Ireland, and in "no way connected with St. Andrews." This the Commissioners consider objectionable, and they assert that "there is no occasion "for persons educated in England to resort to St. Andrews for "a Medical Degree, as a licence of any of the London Medical "Corporations suffices to admit them to practice; and, if they "aspire to a degree, it appears more natural that they should "present themselves as candidates to the University of London."

The right of the University of St. Andrews to confer Medical Degrees upon candidates who have not kept terms there, but who have been a certain time in actual practice, or who hold a recognised diploma in medicine or surgery, is admitted.

That this power may be exercised with advantage to the profession of medicine is also admitted, and that the recipients of the degree are men of recognised worth is shown by the fact that Graduates of St. Andrews are attached, as Lecturers, Physicians, or Surgeons, to nearly all the Schools of Medicine and Hospitals in London and the large towns of the kingdom.

That such a power is of advantage to the public necessarily follows.

But this power should be confined within due limits, and

should be exercised, as the Commissioners say, with "due regard "to the credit of the University and the interests of the public." And so they ordain certain regulations, in order that the Degree may be conferred on none but "persons possessed of sufficiently "high qualifications."

It would follow then that the provisions regulating such a power, advantageous alike to the profession and the public, should not only exclude all unworthy persons, but should also exclude no worthy candidates.

This cannot be done by a limitation in the number of Degrees which the University is empowered to grant annually, for it may well happen that in certain years more than the specified number might be found worthy of the Degree, some of whom must, by by this restriction to ten, be rejected, and so not only an injustice be done to individuals, but an injury inflicted on the University.

It would seem to be just to require of candidates for the Medical Degree who have not kept terms at a University that they should in lieu thereof show that they have been a certain time in the actual practice of their profession; but the present requirement of, practically, nineteen years is much too long. All the advantages of such a provision would be obtained by requiring candidates to show that they have been for five years possessed of a Medical or Surgical qualification, such as would entitle them to be registered under the provisions of the Medical Act.

For such persons a searching and extended examination should be accepted as the sufficient test of qualification for a Degree. The Commissioners have provided a staff of examiners to test the knowledge of candidates in all the branches of Medical Science, and the power of visitation of examinations possessed by the General Council of Medical Education and Registration of the United Kingdom ensures the honest and faithful execution of their duty.

The Commissioners are in error when they state that there is no occasion for persons educated in England who aspire to a Degree to resort to St. Andrews, as they may present themselves to the University of London. The regulations of the latter University do not provide for the cases of Medical Practitioners, who, after a successful career of some years, are desirous of obtaining the degree of M.D., inasmuch as an attendance on several courses of lectures at a recognised Medical School is required, between the first and second examinations for the Degree of M.B.; a condition, obviously, generally incapable of fulfillment by a person in actual practice.

St. Andrews is the only University where such men can obtain a Degree.

By the present regulations many persons well qualified for the Degree of Doctor of Medicine are prevented from obtaining it at a time when it might be of service to them.

If the minimum age at which candidates might offer themselves were reduced by requiring eight years of actual practice, and the number admitted were only limited by the stringency of the examination, the credit of the University, the interests of the public, and the honour of the Degree would be sustained, while no unworthy person would be able to graduate.

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